Candidate POL-1934 Ben Clark Fitzwilliam College Supervisor: Dr Barak Ariel

REASSURANCE CONTACTS BY LOCAL POLICE OFFICERS WITH VICTIMS OF VEHICLE CRIME AND CYCLE THEFT: A BLOCK RANDOMISED CONTROLLED TRIAL

Submitted in part fulfilment of the requirements for the MSt in Applied Criminology and Police Management

Word Count: 17899

February 2021

RESEARCH CONTRACT

Thesis title

Reassurance contacts by local police officers with victims of vehicle crime and cycle theft. A block randomised control trial.

Research Question

Do follow-up phone calls to victims of volume crime offences which have been screened out from further investigation produce higher levels of satisfaction with police when compared to victims who did not receive follow-up contacts?

Research Design

The trial took the form of a block randomised control trial based on crime type reported by victims.

Data & Methods

The study examined the satisfaction of victims who had reported vehicle crime or cycle theft where investigations into their crimes had been closed after the initial report was made to police.

Two Blocks, based on the reported crime type, were created and after exclusion criteria were applied victims were randomly assigned into treatment and control groups within these blocks. In Block A (Vehicle Crime) 762 victims took part – with 371 in the treatment group and 391 in the control group. Block B (Cycle Theft) was slightly smaller with 564 victims divided between 284 in the treatment group and 280 in the control group.

Whilst all victims received the standard service given to victims of crime, those in the treatment group were telephoned by a police officer based in the area where the crime had occurred. The officer explained the screening process, offered reassurance that local police were aware of the crime, explained that local police would tailor patrols if needed and provided crime prevention advice to prevent repeat victimisation if warranted. It was this telephone contact that formed the intervention that was being tested.

Following the intervention, victims from both treatment and control groups were blind surveyed and asked questions to assess their satisfaction with the police in respect of their crime. The survey response rates allowed for a 90% (+/-5%) level of confidence in the responses. Analysis was conducted on an intention to treat basis – with all the responses in treatment group being analysed whether victims had been treated or not in order to allow for accuracy and operational reality. Responses to questions were analysed descriptively (n, mean, sd) and for statistical significance (P=0.1) with 2 tailed T-Tests and for effect size with Cohen's d.

Pre-planned subgroup analysis was also conducted in respect of certain demographic characteristics of victims in order to ascertain whether there was a concentration of effect. The demographics analysed were: age, gender, self-defined ethnicity and repeat victimisation status. The same analytical methods were used in respect of descriptive analysis, significance and effect size.

Findings

Telephone follow-up contact increased victim satisfaction in respect of cycle theft, but not for victims of vehicle crime. Victims in the treatment group for cycle theft reported consistent and statistically significant increases in satisfaction across the majority of the responses to the survey questions. In respect of some of the responses, the effect size was medium to large. This effect, however, was not seen in respect of victims of vehicle crime where there were, at best, negligible effects. In fact, and although not statistically significant, in some cases the effects on vehicle crime victims were negative – meaning a backfire effect may have occurred.

The sub-group analyses revealed concentration of effects in certain groups with medium to large effect sizes seen, but the sample size of these subgroups may limit the accuracy of these findings.

Conclusions and Policy implications

Positive benefits appear to be predicated on crime type. It appears probable that this is due to differing expectations of victims between these crime types, but further research will be needed to confirm or refute this theory. The sub-group analyses also indicate a potential opportunity to close a satisfaction gap in respect of non-white victims and of young people. Replication of the trial is recommended in order to examine the effect of the intervention on victims of other crime types or in respect of particular demographic characteristics of victims.

The results of the trial show there is no such thing as a 'one size fits all' approach to improving satisfaction and any strategy by police agencies in this area must be tailored to crime type and, potentially, also nuanced in respect of victim characteristics. The results of the trial show that there are opportunities to improve victim satisfaction through both a low cost and simple to implement method of contact. However, prior to any large scale implementation, further research is recommended to allow interventions to be targeted at those victims or crime types where the most impactful benefits on satisfaction may be realised.

Acknowledgements

Without the help and support of a number of people, I would not have been able to complete this work and their assistance has been truly invaluable in producing this thesis. There are too many to name here, but my particular thanks goes to Dr Barak Ariel who has provided his guidance, support and expertise throughout the thesis writing process.

I am grateful to the officers at Central North BCU, particularly Chief Superintendent Raj Kohli of Central North BCU who endorsed the trial and allowed it to take place on his command. Chief Inspector Pete Dearden and Inspector Dave Porter worked tirelessly to ensure that the interventions and surveys were completed to time and quality - they went above and beyond the call of duty to ensure that the trial was completed, and I cannot thank them enough.

Finally, no words can say how truly thankful I am to my family – Beccy and Florence. They've missed out on so much family time and their support and encouragement throughout has been amazing and unwavering.

Abstract

The trial assessed the impact of telephone contact by police in respect of satisfaction for victims who had reported vehicle crime or cycle theft but where the crime investigation had been closed. Two blocks, based on crime type, were created. After exclusion criteria were applied, victims were randomly assigned into treatment and control groups within these blocks. Whilst all victims received the standard service given to victims of crime, those in the treatment groups were telephoned by a local police officer who offered updates and crime prevention advice. After this intervention, victims from both treatment and control groups were blind surveyed and asked questions to assess their satisfaction with the police in respect of their crime. Responses to questions were analysed descriptively, for statistical significance and for effect size. Pre-planned subgroup analysis was conducted to ascertain whether there were concentrations of effect. The trial found that telephone follow-up contact increased satisfaction in respect of cycle theft victims, but not for victims of vehicle crime. Victims in the treatment group for cycle theft reported statistically significant increases in satisfaction across the majority of the responses to the survey questions but this was not seen in victims of vehicle crime where there was a negligible and, in some cases, negative effect. Sub-group analyses revealed potential concentration of positive effects in certain age groups and in respect of non-white victims. The results have implications for police agencies who are considering strategies in respect of improving crime victim satisfaction. Further research is recommended to further understand the relationships between crime types, victim characteristics and satisfaction with the police.

Contents

Bibliography	74
Appendix A: Briefing for officers conducting interventions	82
Appendix B: Survey Script	88
Appendix C: Statistical significance levels of questionnaire responses	103
Appendix D: Sample sizes, mean responses & standard deviation results for sub-groups	104
Appendix E: Self-Defined Ethnicity classifications	110
Appendix F: Sample Consort Flow Chart and Checklist	111

List of tables

Table 1: Population and crime comparisons in trial locations	24
Table 2: Reported crime in Camden & Islington 2020	25
Table 3: Follow-up survey completion rates	38
Table 4: Gender splits - Block A and B	40
Table 5: Means (and standard deviations) from questionnaire responses	49
Table 6: Effect sizes with confidence intervals	50
Table 7: Sample sizes of sub-groups for analysis	53
Table 8: Statistical significance and effect sizes of treatment by gender	54
Table 9: Statistical significance and effect sizes of treatment by age group - Block A	56
Table 10: Statistical significance and effect sizes of treatment by age group - Block B	57
Table 12: Self-defined ethnicity by Block	60
Table 13: Statistical significance and effect sizes of treatment by self-defined ethnicity	61

List of figures

Figure 1: Consort Flow Chart - Block A	
Figure 2: Consort Flow Chart - Block B	
Figure 3: Gender ratios Block A vs Block B	
Figure 4: Age spread: Block A vs Block B	
Figure 5: Repeat victimisation levels: Block A vs Block B	
Figure 6: Self-defined Ethnicity: Block A vs Block B	
Figure 7: Relative Difference of effect size - Block A	
Figure 8: Relative difference of effect size - Block B	
-	

Introduction

The Metropolitan Police Service (MPS), like most UK police forces, undertake regular surveys of crime victims to scrutinise the effectiveness of the service provided to them (MOPAC, 2020b, Stanko and Bradford, 2009). The need to maintain acceptable standards of victim care is enshrined in English law through the Victims Code of Practice which requires forces to take certain actions to support victims (Ministry of Justice, 2015). However, these requirements are largely limited to those crimes which warrant further investigation, or where specific risk factors such as hate crime or domestic abuse are present. The majority of victims of crime will not fall into these categories and will not receive much in the way of interaction with police beyond the initial reporting process. The impact of austerity additionally means that forces have moved to online or telephone crime reporting processes (HMIC, 2014). Consequently, victims of crime have a brief and at best, transactional, conversation with a police employee when giving their report (Jackson et al., 2012). Victims in London who said they were satisfied with the police handling of their investigation have fallen from 77% in 2017 down to 67% in 2020 and this fall is even more acute in respect of volume crime reported online or by phone, where levels currently stand at 42% (MOPAC, 2020b). It is important, therefore, to understand how police agencies might intervene to stop or reverse this trend. This trial tested an intervention aimed at improving satisfaction levels for victims of volume crime. An evidence-based approach to target them and test a potential solution to improve their satisfaction was undertaken in the form of a randomised control trial (Sherman, 2017, Sherman and Strang, 2019). Focussed telephone calls were made to randomly selected victims and the impact of this call on their satisfaction levels were assessed through the use of blind surveys, which were then analysed. The findings have the potential to inform policing strategies in respect of delivering satisfaction improvements for victims of volume crime.

Chapter 1: Literature Review

The review begins by clarifying and defining satisfaction as being separate from other concepts such as trust and confidence in the police. The impacts of fair treatment, personal contact and procedural justice as potential drivers of satisfaction are explored and negative influences on satisfaction are identified. Satisfaction levels in London for volume crime currently stand at a record low of 42% (MOPAC, 2020b). Mitigation through telephone follow-up contact with victims is posed as a potential solution to this issue, with positive examples of the use of similar telephone contact in healthcare settings identified as exemplars of improved satisfaction levels. The impact of victim demographics such as gender and race in terms of influencing satisfaction with police are also examined. The review then poses the notion that focussed telephone follow-up contact with victims of volume crime may provide an opportunity to improve satisfaction levels with the police.

Differentiating trust, confidence and satisfaction in policing

Confidence, trust and satisfaction are terms that are often used together as a concept within the policing world, but each have different nuances and interpretations (Cao, 2015). Whilst being clearly interlinked with each other, it is nevertheless important for police leaders to understand that there are differences between them (Holdoway, 2010). The majority of the public generally have little contact with the police and so confidence and trust are often by driven by factors outside the direct control of police agencies. As such confidence and trust are broad, global concepts (Luhmann, 1988). In contrast, satisfaction is derived from the direct experience of the individuals themselves (Jackson and Sunshine, 2007, Skogan, 2006). Satisfaction is therefore an area where the police as an organisation, through the actions of individual officers or staff, can directly affect the experience of those they engage with and therefore affect their levels of satisfaction with those actions (Avery et al., 2020, Bradford et al., 2009).

In terms of citizen-initiated contact the majority of these engagements take the form of reporting crime as a victim to the police (Jackson et al., 2012). It follows, therefore, that victims who report a crime are likely to have their satisfaction with police shaped by the experience that they have in this crime reporting and subsequent investigation process. In turn, this may provide an experiential foundation for their wider perceptions of satisfaction with policing (Dukes et al., 2009, Rosenbaum et al., 2015). There is, however, an unfortunate and demonstrated link between victim initiated contact and subsequent negative views of satisfaction with the police (Bradford, 2010, Jackson et al., 2012, Lee et al., 2019). The magnitude of this negative effect appears to be dependent on how the victim rated the service offered by police forces (Jackson et al., 2012). Key amongst this was the perception of the victim as to how they were treated by individual officers or staff (Bradford, 2010, Bradford et al., 2009, Laxminarayan et al., 2013). This is a concerning fact, and one that UK police forces can address by taking effective steps to improve the quality of service provided and by ensuring fair treatment of victims as part of their wider victim support strategies (Tyler, 2004b). If police agencies can accomplish this, and notwithstanding the benefits this may bring to individual victims, they may well be able to positively influence wider views of police performance among victims of crime (Bradford, 2010, Sunshine and Tyler, 2003). This is due to the fact that when victims perceive higher levels of satisfaction with police they, in turn, report improved wider confidence from that contact (Bradford et al., 2008, Rosenbaum et al., 2015). Conversely, the opposite is true and where victims feel dissatisfied, their reporting of their negative experience is likely to spread via their social networks to a wider audience, thus potentially damaging broader confidence and trust in the police (Goudriaan et al., 2006).

Fair treatment and procedural justice as drivers of satisfaction

A repeated aspiration raised by victims when considering their attitudes towards police is their desire to be treated fairly and have their voice listened to by an officer (Bottoms and Tankebe, 2012, Bradford, 2010, Hinds and Murphy, 2007a). The sense of fairness that such reassurance and engagement with victims can provide is a key determinant not just of satisfaction, but also of wider confidence and legitimacy with police as a whole across communities (Elliott et al., 2012, Hinds and Murphy, 2007b). In discussing police legitimacy, Justice Tankebe has highlighted four factors of lawfulness, effectiveness, procedural justice and distributive justice as drivers of legitimacy and thus confidence (Tankebe, 2014). By ensuring that a sense of effectiveness and procedural justice is felt by victims when reporting crime, the impact of negative influences on satisfaction with police from more global influences, such as critical media reports, may be able to be reduced (Hu et al., 2020).

Developing this theme of procedural justice; victims have consistently reported that 'solving the case' is not the most important factor in gauging their satisfaction (Hinds and Murphy, 2007b, Jonathan-Zamir et al., 2015, Mazerolle et al., 2013). Indeed, for the majority of victims of crime, success in terms of a detection and conviction of an offender simply does not occur, with detections currently standing at 7.8% in England and Wales (BBC, 2019). In actual fact, the way police spoke to and engaged with victims often meant more to them and helped to deliver the sense of fairness they sought (Bradford et al., 2008, Elliott et al., 2012, Jonathan-Zamir et al., 2015). Reports from victims are clear that they rate their sense of satisfaction not just on the outcome of a case, but on how they were treated (Tyler, 2004a). This point of 'fairness' and procedural justice being allowed to happen is important to citizens (Tyler, 1988, Tyler, 2003). A study of Ombudsman outcomes in Germany showed that participants expressed positive views of the process even where decisions did not go in their favour. These participants felt that the process had reached a just and natural conclusion with their concerns being listened to by authority figures and thus they remained satisfied with the process (Bradford and Creutzfeld, 2018). It therefore appears important that police agencies need to consider how they treat victims of crime through their reporting and investigation processes. If such systems are implemented that allow victims to feel that the police have supported and listened to them, then they will likely feel that procedural justice has occurred and that they have had their chance to be listened to (Tyler, 1988). It follows, therefore, that it will also be likely that their satisfaction levels will improve in consequence.

Satisfaction and volume crime

When considering implementing such systems, there is of course the challenge that the numbers of crimes recorded could overwhelm police agencies efforts to engage with victims. With a need to prioritise, forces are rightly focussed on targeting higher harm crimes (Dudfield et al., 2017) but a consequence of this is that many police forces have struggled to manage their performance in respect of volume crime (Jansson, 2005). London is a microcosm of this issue with large volumes of crime and reducing levels of satisfaction from victims of late (MOPAC, 2020a, MPS, 2020c). How police forces manage volume crime has had a degree of research associated with it, but this research has tended to focus on links with serious crime (Brown and Smith, 2018) or intelligence gathering opportunities (Cope, 2004) rather than around improving the victim experience per se. Jansson (2005), reviewed the literature around the management of volume crime and suggested that improvements in initial responses to volume crime calls were a potential means to improve performance outcomes (Jansson, 2005). Whilst this is desirable, the impact of austerity has meant that many forces have had no choice but to curtail such activities in order to focus on threat, harm and risk (HMIC, 2014). It also follows that due to increased paucity of police resources, crime that occurs in volume is less likely to be detected as the opportunities for an initial response to identify solvability factors is reduced as well (Coupe, 2016). Indeed, this is borne out by the Office for National Statistics, which have reported overall detection rates reducing year on year, with a low of 7.8% last year (BBC, 2019).

However, as already noted, victim satisfaction is not merely dependent on detections and can be influenced by other means. This therefore presents potential opportunities for police agencies to redress the balance. It has already been shown that receiving a sense of fair treatment and procedural justice is as important to a victim of volume crime as detecting the crime is (Bradford, 2010, Bradford, 2011) and so in the face of constrained resourcing, forces should consider how their strategies for supporting victims can deliver this (Tyler, 2004b). Studies have shown that where victims do not receive personal contact or reassurance from police, then their satisfaction levels are likely to drop and their wellbeing may be adversely affected (Laxminarayan et al., 2013, Maddox et al., 2011, Norris and Thompson, 1993). In London, the Metropolitan Police Service (MPS) has moved the majority of its' crime reporting online, adopting a standard 'Crime Assessment Policy' which details which crimes will be closed after initial online or telephone investigation with minimal personal contact with victims (MPS, 2017). In the three years since this process was adopted, there has been a drop in satisfaction from victims with overall satisfaction rates standing 10% lower than they did in 2017 (MOPAC, 2020b). Whilst some of this difference can be explained by changes in methodology in respect of survey data (MPS, 2020c) it appears possible that the twin issues of an impersonal service and limited opportunities for detecting crime may be a factor in driving this downward trend in victim satisfaction.

How reassurance activity can influence satisfaction

Bradford (2010) states that treatment given by police to victims should provide a sense of reassurance in order to enhance perceptions of fairness and thus improve their satisfaction. (Bradford, 2010). An important element of reassurance activity is the use of community-based or local police officers to help engender a feeling of neighbourhood safety among citizens (Dukes et al., 2009). As part of an evaluation of activities that police undertook in terms of victim reassurance, a UK study found that by using local officers to engage with them, satisfaction and confidence levels of victims at the ward level could be improved (Tuffin et al., 2006). In the London policing context, the use of neighbourhood or locally based officers to provide reassurance already forms part of daily policing activity (Bradford, 2010). Burglary victims in London have significantly higher satisfaction levels than volume crime victims – with 77% of burglary victims reporting satisfaction against 48% of those who have reported other crimes online but detection rates in both types remain low (MOPAC, 2020b). The difference however is that, in the MPS, locally-based officers are tasked to visit burglary victims to offer reassurance and direct personal contact (Chenerey et al., 1997). This would appear to support the premise that local officers delivering reassurance activity deliver benefits in terms of satisfaction.

Telephone contact and satisfaction

A number of studies have confirmed that focussed follow-up contact with victims offers reassurance and can prevent some of the adverse effects of victimisation, therefore improving satisfaction (Chenerey et al., 1997, Forrester et al., 1988, Shapland and Hall, 2007). However, in these studies, follow-up contact took the form of personal visits - a tactic that is too resource intensive to be replicated for volume crime victims in the current UK policing context. Is it possible, therefore, that telephone contact can deliver similar benefits in terms of satisfaction improvement to crime victims but without the impact on resources? There have been few criminological studies in this regard, and the results of those that have taken place are mixed. Some, such as a study in San Jose, focussed on victim recall rather than satisfaction per-se (Turner, 1981) whilst others looked at using call-backs for reassurance but were focussed more on long-term problem solving rather than satisfaction (Skogan and Wycoff, 1987). Neither study looked at the experience of volume crime victims and, due to the age of these studies more recent factors, such as the impact of online reporting, would not have been considerations. Although there have been few studies in Criminology around whether telephone contact can improve satisfaction, that is not necessarily the case in other settings. One of the most studied fields is that of public health and there are a number of example studies that focus on precisely this issue: A study of post-operative day-surgery patients found that patient satisfaction and, consequently, health outcomes were improved by follow-up telephone contact (Daniels et al., 2016). In another example, nurse-led call-backs to sleep apnoea patients resulted in higher levels of patient satisfaction coupled with increased efficiency of resources (Walijee et al., 2020). Additionally, and of importance when considering the impact of austerity, it appears that for patients telephone followup is as effective as personal follow-up and is easier to manage from a managerial and logistical viewpoint (Mathew et al., 2017). What is worthwhile noting from these studies is not only that they cover a variety of settings and circumstances, but they all seem to show promise in terms of patient satisfaction and wellbeing.

Reflecting on these findings from the field of public health, there is promise that these benefits from follow-up calls can be replicated in terms of crime victims: In a study of fraud victims, it was found that those who received telephone support after reporting crime reported greater satisfaction coupled with a reduced propensity to repeat victimisation (Cross, 2016). This, and the experience of the healthcare sector shows that follow-up telephone contact can be beneficial and yield improved satisfaction for minimal resource outlays.

The influence of demographics on victim satisfaction

Various studies have highlighted that there is no 'one size fits all' approach to victim satisfaction – with factors such as age, race and gender all affecting how victims may perceive police actions (Aihio, 2017, Chandek and Porter, 1998, Kule et al., 2019). As such, it is clearly worth considering these demographics in terms of any sub-group analysis and identifying if there are variances or concentrations of effect within them.

When considering race, studies have shown that there are differences in reported satisfaction between white victims and BAME (Black and Minority Ethnic) victims. BAME victims consistently report lower levels of satisfaction with police (Circo et al., 2019, Dai and Johnson, 2009, Desmond et al., 2016). Whilst most of these studies have taken place in the United States, when the issue has been examined in the United Kingdom not dissimilar findings have also been found (Barrett et al., 2014). MPS and the London Mayor's Office for Policing and Crime (MOPAC) public voice data show that there are current disparities in satisfaction between white victims and BAME victims (MOPAC, 2020b). Whilst overall satisfaction levels stand at 67% of victims saying they are satisfied or very satisfied, this figure masks differences where race is concerned (Kule et al., 2019). In the latest survey 71% of those who identified as 'White British' reported that they were satisfied, 69% of 'Black' victims and only 61% of 'Mixed' victims reported similar views. Coming at a time when commentators, including former senior BAME officers, are publicly stating that race relations between the black community and the MPS are at their lowest known levels (Nestor, 2020) the need to improve BAME satisfaction is acute.

The same MPS and MOPAC data also show similar disparities when age is considered – with younger people and female victims reporting lower satisfaction levels (MOPAC, 2020b). This supports the theory that younger people are less likely to be satisfied with police actions than older people (Brandl and Horvath, 1991). There is also evidence that gender may also play a role – with female victims likely to have increased satisfaction as a result of reassurance activity by police (Foley and Terrill, 2008). A final point worthy of consideration, and therefore potential analysis, in respect of victim demographics and experiences are those of repeat victims who consistently report that they are less likely to be satisfied with police responses than first-time victims of crime (van Dijk, 2001).

Can follow-up contact influence victim satisfaction?

Bringing the considerations that drive satisfaction together; tactics to deliver improved victim satisfaction may also improve police legitimacy in those communities where trust in the police is low (Cao, 2015). By demonstrating procedural justice and fairness in victim engagement, police can take clear and visible steps to improve satisfaction (Jackson and Sunshine, 2007). As Ben Bradford in his study of victim satisfaction in London acknowledges – it is unlikely that police will be able to provide detection outcomes in respect of volume crime, so if satisfaction is to be influenced, it must be through other means (Bradford, 2010). It appears that the use of online reporting and screening processes to manage volume crime may have had a negative impact on victim satisfaction (MPS, 2020c), but it appears that the use of local, community-based officers to deliver reassurance messages can help to offset this and improve satisfaction levels among victims, especially in diverse communities (Chenerey et al., 1997, Dukes et al., 2009).

With the experience of other fields showing that follow-up telephone contact can be beneficial and yield benefits for minimal cost outlays, it therefore appears appropriate to test whether telephone follow-up contacts by local police offering a level of personal contact and reassurance can improve satisfaction levels for victims of volume crime.

Research Question

This trial aimed to test, therefore, whether telephone follow-up phone calls to victims of volume crime offences produced higher levels of satisfaction with police when compared to no-treatment victims who did not receive personal follow-up contact.

Chapter 2: Methods

Trial Overview

The trial took the form of a block randomised control trial with a sample population of victims of crime that had been screened out from further investigation. The trial consisted of two parallel treatment and control groups within two discrete crime type blocks: Block A related to victims of motor vehicle crime and Block B related to victims of pedal cycle theft. By blocking the victims according to crime type, the inter-block variability was assumed to be lower than the intra-block variability, therefore increasing the precision of the test (Ariel and Farrington, 2014).

In total, 1694 victims in either of these two crime types were recorded during the study period. However, given the study's eligibility criteria, only 1326 victims were then randomly assigned into treatment and control conditions within each block. The most common reasons for treatment loss were crimes which were screened-in for investigation, where inaccurate telephone numbers had been recorded or where no contact details had been recorded for the victim. After eligibility assessment a randomisation process took place, with allocation between treatment and control groups in each block in the approximate ratio 1:1.

Victims in both control groups received the standard service provided by the MPS for victims of crime which had been screened out from further investigation. This consisted of a letter confirming that the crime had been reported had been recorded but that no further investigation would be conducted at that stage due to insufficiency of evidence, not being in the public interest or due to the MPS Crime Assessment Policy (MPS, 2017). However, victims in the treatment group received an additional call-back from a locally-based officer who offered a verbal update in respect of the investigation; provided reassurance that the police were aware of the case and would tailor patrols where necessary and, where appropriate, offered crime prevention advice as well. It was this telephone conversation which served as the tested intervention and thus acted as the independent variable in each block.

To measure the effect of the intervention, victims from both the treatment and control groups were surveyed by a (different) team of officers who asked the participants a series of questions about their satisfaction in respect of the police response to their crime. These questions were drawn from existing MOPAC victim satisfaction surveys (MPS, 2020c) and consisted of a mixture of 3 and 7-point Likert Scale questions, together with questions covering basic demographic information.

Following the survey, intention to treat (ITT) analysis of the survey responses was undertaken to assess the relative effectiveness of the intervention (i.e. the telephone call-back) by comparing each question across the groups. Analysis consisted of descriptive analysis, statistical significance tests (independent samples t-tests) and measures of magnitude of effect size (Cohen's *d*). Further analysis was also conducted on sub-groups of participants based on certain demographics: age, race, gender and repeat-victimisation - although the analysed sample size (total n=498) does not allow for firm conclusions to be drawn from these subsets.

Participants

Participants were victims who had reported a crime to the police over a ten-week period from 8th August 2020 through to 17th October 2020 where their crime had not been screened in for further investigation. The trial concentrated on two crime types that are often closed without further investigation beyond initial assessment: motor vehicle crime (i.e., theft from motor vehicle, theft of motor vehicle, criminal damage to motor vehicle, and criminal attempts connected to the previous three offences) or pedal cycle thefts (i.e., thefts of cycle and criminal attempts connected to cycle theft). The trial examined crimes that had taken place in the London Boroughs of Camden and Islington, known by the MPS as the Central North Basic Command Unit (CN BCU). Each reported crime was allocated a unique crime reference number by the MPS on its Crime Report Information System (CRIS) and for the purpose of this trial, the CRIS number served as a unique reference number for each

victim. 1,694 victims were initially identified as being potentially eligible in the two study blocks before eligibility criteria were applied.

However, the overall pool of participants was diminished by 22%-24% because of ineligibility. Detailed in the flowcharts for each block (see Figures 1&2), 311 cases were lost because further investigation was being conducted or where factors such as hate crimes and domestic abuse existed (n = 192 in Block A, n=176 in Block B). These cases followed a policy of mandatory contact by the police with the victims as part of a risk management process and thus could potentially confound the treatment. An additional 42 cases were excluded where no contact details were listed in the police report to administer the intervention over the telephone. Two additional victims who announced at the time of the initial reporting of the crime their wish not to have any further contact with police were screened out as well. As also shown in the flowcharts, the removal of cases followed the same criteria in both blocks and took place before the random assignment or administration of the treatment.

Additional cases were lost post randomisation, due to police policy criteria that was not considered in the experimental protocol. For example, when further evidence had caused the investigation to be reopened (n=10), and for 7 victims who are unable to converse on the telephone (e.g., hearing impairment) there are different investigation schemes, again potentially acting as a confounding variable. Furthermore, victims under the age of 16 years at the time the offence was reported (n=8) were also screened out post random assignment, as the requirement to involve the guardian in the contact and their consent to participate in the survey proved too cumbersome for the purposes of this trial.

Changes to intervention methodology (introduced pre-intervention)

The research proposal had envisaged using police officers and police staff from local Safer Neighbourhoods Teams (SNTs) to conduct the call-backs. SNTs are ward based and part of their core role is being accessible to their local communities to conduct engagement and reassurance activities (College of Policing, 2020). However, it was decided following pre-testing to use officers based in the BCU Operations Centre instead. This was a relatively small cadre of officers working under one supervising officer and thus the briefing and implementation process was greatly simplified. It also helped officers on the team to raise issues as part of a regular review process and exercise a feedback loop through this single line manager (Neyroud, 2019).

The second change was to reduce the period from which crimes were selected from a planned 12-week trial – set to run from 1st August 2020 through to 24th October 2020 - down to a 10 week period – running from 8th August 2020 to 17th October 2020. This was due to two main reasons – a delay at the beginning of the trial period in setting up the correct search parameters on the MPS Crime Reporting (CRIS) system mean that the 1st August deadline could not be met and, at the tail end of the trial, operational commitments for the BCU based officers in respect of both the COVID19 policing response and seasonal crime reduction activities meant that commitments to the intervention process could not be met after the week commencing 18th October without incurring overtime costs, for which the trial had no budget. A decision was therefore made not to include crimes reported on or after 18th October 2020.

Consideration was given to pausing interventions and then resuming them once these operational pressures had eased, but given the unpredictable nature of the COVID19 pandemic it was decided to halt the trial rather than pause it. Indeed, shortly after this date, the second National lockdown was announced (BBC, 2020) which appeared to vindicate this decision to halt the trial at that point.

Although the reduction in the trial time was not desirable there was a side-benefit in that it became possible to use the period of 1st August through to 8th August to enhance preparation work prior to go-live. This was done through testing the intervention processes and ensuring the officers involved were personally briefed (the initial plan had simply been for a written briefing) with the mechanisms for officer feedback being clearly developed. By taking this unexpected opportunity, the

trial was able to use these proven methods to increase the chance of a successful implementation (MacQueen and Bradford, 2016).

Settings

The trial was conducted in the London Boroughs of Camden and Islington, which collectively form the Central North Basic Command Unit (CN BCU). There were two reasons why CN BCU was chosen as the trial site – firstly, the relatively high levels of motor vehicle and cycle crime indicated that achieving sufficient dosage during the trial period was more likely than in some other London Boroughs, something that would be necessary in order to enable a realistic conclusion to be drawn from the samples (Ariel, 2019). Secondly, the author was (at the time of the trial) in the direct command chain in respect of policing the BCU. This meant that a successful implementation process was likely to be more achievable, as there were clear avenues to both communicate with and receive feedback from officers engaged in the trial (MacQueen and Bradford, 2016).

In terms of the population profiles the two Boroughs that comprise CN BCU have some of the highest population densities in the UK, with a 2017 population estimate of 242,500 residents for Camden and 231,200 residents for Islington (see Table 1). Islington has the second highest population density in England & Wales, with density in Camden being marginally lower due to the presence of large open spaces such as Hampstead Heath and Primrose Hill (London Borough of Camden, 2020). Both Boroughs have a gender split that is virtually identical to the average for England & Wales. The median age in each Borough is slightly below the London median, with Camden having a slightly older population than Islington. Both Borough populations are significantly below the median age for England & Wales. (Greater London Authority, 2020, London Borough of Camden, 2020, ONS, 2020).

TABLE 1: POPULATION AND CRIME C	COMPARISONS IN TRIAL LOCATIONS
--	--------------------------------

	Camden	Islington	London	England & Wales
Resident Population	242,500	231,200	275,831	-
Population Density per Hectare	111.3	155.6	56.2	+
Median Age	34.0	31.9	36.0	40.0
Gender Split (% male/% Female)	50/50	49/51	49/51	49/51
Self-defined BAME (%)	34.6	32.0	42.5	-
Overall Crime Rate/1000	123.5	121.2	84.0	65.7
Theft from motor vehicle/1000 population	10.57	9.15	9.01	-
Theft of cycles/1000 population	6.38	5.76	2.54	-

In terms of self-definition of ethnicity in respect of the 18-point scale as used in the 2011 Census (See **Appendix (D)**), the census data shows the proportion of those self-defining as non-white in both Boroughs to be below the London average level. However, it should be noted that both Camden and Islington have seen significant 'non-white' population increases since 2011 and it is likely that these proportions may now be different (ONS, 2018b).

Camden and Islington have significantly higher crime rates than the average in London and almost double the rate per thousand than the average in England & Wales (ONS, 2018a). CN BCU has significantly higher levels of thefts of cycles than the London average and, whilst Islington's levels of vehicle crime are in line with the levels seen in most other London Boroughs, levels of vehicle crime in Camden are slightly higher than in other London Boroughs (MPS, 2020a).

Over the study period, Table 2 outlines the numbers of crime reports recorded in the two Boroughs over a ten-week period prior to 18th July 2020 and over a rolling 12-month period up to that date (MOPAC, 2020a, MPS, 2020a). From these data it was reasonable to draw the conclusion that a 10-week trial period had the necessary base rate levels (Hinkle et al., 2013). It should be noted that the COVID19 pandemic had impacted on crime levels in the months preceding the trial, with recorded crime levels falling by up to 32% between March and May 2020 which was the time of the first national lockdown (Stripe, 2020). However, by the time the trial started, the effects caused by this lockdown had largely dissipated and crime rates were on a par with the previous year's levels (MPS, 2020a).

TABLE 2: REPORTED CRIME IN CAMDEN & ISLINGTON 2020

	10 weeks up to 18/07/2020	Rolling 12 months
Theft of Pedal Cycle	753	2547
Theft from Motor Vehicle	777	4564

A final factor to note is that in terms of policing, the two Boroughs are brigaded together under one Basic Command Unit. The officers involved in administering interventions to the treatment groups worked from a central operations room on the Command Unit. This meant the officers involved in the trial, although locally-based, were not aligned to any particular Borough and the treatments administered did not differ between the two local authority areas. Additionally, the different officers who conducted the surveys were again officers aligned to the Command Unit Headquarters rather than to any particular ward or local authority area.

Interventions

Before considering the detail of the intervention for the treatment group, it is important to outline the 'business as usual' processes common to all groups of victims in the trial (MPS, 2020d). This will help add context to the intervention process for the treatment group as well as helping to understand which of the survey questions were more likely to be affected by the intervention.

The 'screening' process

The crimes were reported in a variety of ways – In person (either at a police station or to an officer in the street), by telephone or online. In the case of online reporting, the victim would complete a simple report and an officer or staff member from the MPS would then call the victim back to complete the

formal report of the crime. Data was not collected in respect of how each individual crime in the trial was reported to police, although one of the survey questions does ask victims to rate their satisfaction in respect of the reporting process.

Once the crime was recorded, it was allocated a unique CRIS number and then a decision was made as to whether the crime would be screened in for further investigation. This was based on solvability factors and the MPS has a formal process for assessing these, known as the Crime Assessment Policy (MPS, 2017). Crimes that were screened 'in' for further investigation would be passed to an investigating officer to make contact with the victim and progress the matter. Under the Crime Assessment Policy certain crimes, such as domestic abuse allegations and those flagged as being hate crimes, are always screened in regardless of solvability factors. This is to ensure that any particular threats and risks to victims are managed appropriately. In all of these cases, an investigator is required to make follow-up contact with the victim and so these crimes were excluded from the trial in order to minimise the risk of confounding variables affecting the outcomes.

For crimes screened 'out' from further investigation, the MPS generates a standard letter to victims detailing the crime reference number, the reasons why the crime has been screened out and what this means together with some basic information around contacting the MPS should further information come to light. Once this letter had been sent, the crime would usually be closed unless further information subsequently emerged that warranted a re-opening of the crime report.

This, therefore, was the process that all crimes in both blocks had been through. The trial intervention for the treatment groups took place after this point had been reached. Once a crime had been reported, a week to ten days was allowed for a screening decision to be made in respect of the crime and for any victim of crime letter to be sent. This pause allowed a screening decision to be made for the vast majority of crimes and it was only in a small number of cases (n=9) that a screening decision was delayed. These cases were excluded from the randomisation process.

Treatment Conditions

Following this pause to allow the screening-out decision to be made, crimes that were randomly assigned into the treatment groups were assigned to a supervisor in-charge of the experimental operations team at CN BCU, who in turn would allocate officers to undertake the intervention process. These cases were normally passed on a weekly basis as a bulk transference, so in some cases up to 2 weeks may have passed before the crime was allocated for intervention.

The trial intervention took the form of a telephone call from an officer based at CN BCU to the victim. All officers involved were briefed about the purpose of the trial, the reasons behind the call and the requirements of the call. Additionally, all officers were instructed to use an aide-memoire during the call (See **Appendix (A)**). The officers were asked to attempt to contact victims between 8:00am and 9:00pm and were asked to make at least 3 attempts to do so. A spreadsheet was provided for them to log this process.

Once contact was made with a victim and the victim was willing to engage, the officer was asked to cover the following main points:

- That the officer worked in the area local to where the offence had taken place
- That the officer had read the crime report and was calling to provide a follow-up contact
- Explain to the victim that the crime had been screened out, but that the information would still be used by police for example for intelligence or for tasking patrol patterns.
- That the local Safer Neighbourhoods Team would be made aware and would use the information to plan their activities in terms of crime prevention and reduction
- To offer any crime prevention advice if information from the crime report or the victim warranted it.
- To offer any victim support if information from the crime report or the victim warranted it.
- To provide contact details for the MPS if the victim required them.

The briefing to officers was not to use a formal script per se but to tailor their conversation and empathise and sympathise as they felt necessary. Officers were also given information in the form of a list of frequently asked questions (see **Appendix (A)**). Officers were reminded not to tell victims about any trial or survey process to avoid affecting the measures (Ariel et al., 2019).

As part of pre-implementation testing (Fixsen et al, 2005), test calls were made to victims which did not form part of the trial. From these calls, it was established that the call backs to victims took between 5 and 10 minutes to complete, with no call exceeding 10 minutes in duration. From this it is possible to estimate the cost of each intervention at £6.79 notwithstanding actual telephony costs. This is based on a police constable's hourly cost of £40.74 (MPS, 2020b).

Once a telephone call had been completed, or if the intervention was not delivered, the data spreadsheet was updated together with any reason why the treatment could not be carried out. Progress was reviewed every 10 days during the trial period at scheduled meetings between the author, senior management team representative from CN BCU and the supervisor in charge of the operations team. These meetings allowed the team to review progress, check for risks or threats to the trial and receive feedback from the operations team.

Outcome measures

Once the intervention process was complete, the trial moved on to the next phase – which was the conducting of a blind survey of both treatment and control groups in each block. The survey process ran 2-4 weeks after the intervention process for any given week in the trial. This was in order to mitigate the risk of inadvertently surveying those in the treatment group prior to the intervention actually taking place.

The survey was administered by officers from CN BCU who were separate from the team who had delivered the treatment. These officers were selected to undertake the survey work by the management at CN BCU and mostly consisted of officers on restricted duties. One of the effects of the COVID19 pandemic was that a few officers were required to work from home on restricted duties due to various vulnerabilities and the necessity for them to shield. This allowed for a pool of officers not connected with the treatment to undertake the survey activity. The officers were blinded as to the reasons for the survey and in which group – experimental or control – the victim was assigned to. The officers engaged in the survey activity were trained about the instrument and that the survey was about assessing victim satisfaction levels. However, they were not told the fact that the survey was part of a trial. The officers engaged in the survey activity were provided with a script (See **Appendix (B)**). Survey responses were entered into a spreadsheet which differed from the sheet used to log interventions.

The survey questions were drawn from the 'MOPAC user satisfaction survey' (MPS, 2020c). These items drive public performance data for the MPS in terms of victim satisfaction and confidence measures (MOPAC, 2020b). The use of these questions was intended to allow easy comparison with existing MOPAC data and to allow for later replications. As the trial was focussing on victim satisfaction, only 9 core questions were asked of respondents. These were a mix of 3- and 7-point Likert-scale questions (Joshi et al., 2015) ranging from 1 (Completely dissatisfied) to 7 (Completely satisfied) or a three-point scale as detailed below:

QUESTION 1: "Thinking about the crime you reported were you satisfied, dissatisfied or neither with the actions taken by the police at the time you reported it? And is that completely, very or fairly (if not 'neither')?"

QUESTION 2: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with any follow up actions from the police? And is that completely, very or fairly (if not 'neither')?"

QUESTION 3: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with how you were kept informed by the police? And is that completely, very or fairly (if not 'neither')?"

QUESTION 4: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with how you were updated by the police? And is that completely, very or fairly (if not 'neither')?" QUESTION 5: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with how you were treated by the police? And is that completely, very or fairly (if not 'neither')?"

QUESTION 6: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with the overall service you received? And is that completely, very or fairly (if not 'neither')?"

QUESTION 7 (3-POINT SCALE): "Did the service you received from the police exceed, meet or fall below your expectations?"

QUESTION 8 (3-POINT SCALE): "Prior to this incident was your general opinion of the police high, low or mixed?"

QUESTION 9 (3-POINT SCALE): "As a result of this incident, has your opinion of the police got better, got worse or not changed?"

Further to these main survey questions, officers were also asked to obtain demographic information on the victims. These included age, gender, self-defined ethnicity and whether the respondent had suffered from crime victimisation in the preceding 12 months. These variables were used for the pre-planned sub-group analyses in order to identify if concentration of effects existed within predefined populations of victims in London.

As with the intervention process, officers were asked to make three attempts to get a response to a telephone call and to log these attempts on the overarching survey spreadsheet. If unsuccessful, the officer who had attempted to conduct the survey was asked to note the reason why on the sheet.

Sample Size

When planning the trial, the crime figures from 2019 were used as a basis for determining the sample size. With a preselected confidence interval of 90% (+/-5%) in responses, an online calculator (SurveyMonkey, 2020) was used to determine the ideal sample size based on the 2019 12-week totals of 753 for Block A and 777 for Block B. The results of this calculation determined that the minimum

sample size to achieve the desired confidence interval for Block A would need to be 201 responses and for Block B it would need to be 202 responses. It was anticipated that at least two-thirds of the randomised groups would be lost to follow up, so responses would need to be n=210 for Block A and n=217 for Block B.

In the event during the trial there were 270 responses for Block A (35.4%) and 228 for Block B (40.4%) (see Figures 1 & 2). In terms of statistical power, a minimum detectable effect size of 0.3 was possible given these block sizes (Ariel, 2019).

Randomisation

Block allocation was based on reported crime type – with vehicle crime being allocated to Block A and cycle theft to Block B. Once allocated to blocks, eligibility criteria for each crime were assessed. Once exclusions were applied, crimes were allocated into treatment or control groups through a weekly batch random assignment with no restrictions using randomisation via Microsoft Excel.

Allocation was conducted by the research team who would then communicate the assignment to the intervention team. Once the interventions had taken place or had been attempted in line with the instructions given, the completed list was checked for quality by the supervisor in charge at CN BCU and then returned to the research team. Data was stored in cloud storage, and the folders containing the randomisation and allocation data were not accessible by anyone other than the research team.

Blinding

In respect of the surveys, the officers conducting the interventions were not told that the survey formed part of a trial. Their script (See **Appendix (B)**) only stated that the survey was to assess satisfaction levels on the BCU. The survey log provided to them did not include information as to whether any particular victim was in a treatment or control group.

Additionally, the briefing note to the officers conducting the interventions (See **Appendix (A)**) specifically requested that no mention be made to victims of follow-up surveys or the fact that a particular victim had been selected to be in a trial. This was also covered in the verbal briefing given to the officers at the beginning of the trial period. Through these mechanisms, both surveying officers and the victims themselves were blinded as to a) the trial process and b) the intervention.

Statistical methods

Analysis was conducted on both blocks on an 'Intention to Treat' basis. Prior to analysis, data were checked to ensure that any duplicate results were not counted. Additionally, where a respondent had indicated an answer to a question other than the 7- or 3-point scale results – for example 'Don't know' or 'Refused to answer' – these answers were discounted for the purposes of analysis, however other valid responses from the respondent were included.

There were three main areas of statistical analysis that were used when analysing the results from the survey questions of both control and the treatment groups (for each item):

- Descriptive analysis was conducted to identify respondent numbers (n), means and standard deviation (sd) in respect of the answers to questions. Furthermore, descriptive analysis of the demographic characteristics of the blocks and groups was conducted.
- Statistical significance two-tailed independent samples t-tests were conducted in both blocks to assess the statistically significant differences between the control group and the treatment group. These tests were run through the Microsoft Excel data analysis add-in pack.
- Effect size Cohen's *d* (Cohen, 1988) was calculated using an online calculator (Becker, 2000).
 In sub-group analysis where n<20 for either the treatment or ITT group was shown then an alternative value for Hedge's *g* (Enzmann, 2015) was also calculated and is listed as a note in the results, however, Cohen's *d* was used as the main measure for consistency.

In terms of statistical significance, the p values are listed to 3 decimal places, but where p values are <0.1 or <0.05, these are highlighted. Effect size is again listed to 3 decimal places, with a 90% confidence interval (+/-5%) used in terms of analysis presentation using an online calculator (Georgiev, 2020). This was considered appropriate for the trial results, given the sample size and the level of confidence that would be suitable in any operational implementation of findings. Although not the standard level of significance, a 10% (p<0.1) level was chosen as this has been shown to be more relevant to operational policing contexts (Sherman and Weisburd, 1995).

Additional Analyses

Further planned analysis took place in respect of sub-groups based on demographic characteristics. The analysis used identical calculations to the main group in respect of effect size and statistical significance as follows:

- Gender: Male v Female
- Race: White (White British/White Irish/White Other) v BAME (all other ethnic groups)
- Age: (24 and under/25-34/35-44/45-54/55 and over)
- Victimisation: Repeat victim in last 12 months vs non-repeat victims.

There are caveats that need to be noted when considering the results of these sub-group analyses. Firstly, the groups were not randomised in themselves – rather they were drawn from the wider population which had been randomised. As such, bias may exist in terms of the population of these sub-groups. Secondly sub-grouping the populations into complementary groups for analysis did, by its nature, produce small sample sizes. Thus, the risk of outliers influencing the responses is magnified (Bland, 2020) and in no case did the population of these groups exceed the levels required to have a confidence interval of 90% like the main group.

Chapter 3: Results

In describing the results the CONSORT flowchart and checklist model has been followed (see **Appendix (F)**) (Schulz et al., 2010). The results section will set the scene by firstly examining the process of participant flow – including descriptions of losses and exclusions – followed by the process for intervention and follow-up surveys. A descriptive analysis of the demographic properties of the participants will be also be outlined. The results will be described in terms of descriptive analysis of means and standard deviation, together with the results of calculations with respect to statistical significance and effect size. Finally, sub-group analyses of both significance and effect size will be described with respect to participant gender, repeat-victimisation status, age and self-defined ethnicity.

Participant Flow, losses and exclusions

There were four phases to the Participant flow in the trial:

- Selection this phase encompassed identifying crimes for allocation into the two blocks and applying any exclusion criteria.
- Randomisation the process of allocating crimes into the control and treatment groups for each block.
- Intervention during this phase, officers attempted to contact victims in the treatment group and apply the treatment in terms of the follow-up call process.
- Follow-up survey- once intervention had taken place, differing officers conducted telephone surveys on victims from both the control and treatment groups.

CONSORT participant flow diagrams for each Block covering all four phases are shown in Figs. 1&2 below (Schulz et al., 2010). These diagrams provide a summary of the participant flow in each phase, together with details of losses and exclusions.

FIGURE 1: CONSORT FLOW CHART - BLOCK A



FIGURE 2: CONSORT FLOW CHART - BLOCK B


Selection

Over the trial period a total of 1694 crimes were recorded. Based on the allegation type these were classified and blocked either as Motor Vehicle crime (Block A) or Cycle Theft (Block B). In total, Block A had 954 crimes (56.3%) and Block B had 740 crimes (43.7%).

Once allocated to either of the two blocks, the crimes were assessed for eligibility and exclusions applied. In the case of Block A, 192 crimes (20.1% of the block) were excluded at this point and in Block B, 176 crimes (23.8% of the block) were also excluded.

Randomisation

Once the exclusion criteria were applied, randomisation took place using the method outlined in the previous chapter. This was a simple randomisation process which divided the blocks into control and treatment groups as shown below. No losses occurred at this stage of the process. No further randomisation took place after this point.

Intervention

Treatment was successfully delivered in 55.8% of cases (n=207) in Block A and 58.8% of cases (n=167) in Block B. There were a number of reasons for treatment loss, but the most substantial is 'unsuccessful contact' - i.e. where contact had been attempted but the victim did not answer the phone after three attempts. It should be noted at this point that the MPS telephone system routinely masks its identity, with calls showing on recipients phones as an 'unknown number'. It appears likely therefore, that this may have been a factor in terms of treatment loss. Although instructions were provided to officers on how to bypass this feature (see **Appendix (A)**), it was not possible to assess whether this was actually done by the officers either conducting the interventions or the surveys.

Other substantial reasons for treatment loss included cases where the number had been recorded incorrectly at the time of reporting and cases which had been re-opened for further investigation after the initial screening decision. There were also a number of other more minor

reasons for treatment loss, including language barriers, refusal on the part of the victim to engage and cases where, on speaking to the victim, the exclusion criteria should have been applied.

Follow-up survey

Once the process of treatment had taken place, attempts were made to survey all victims in both treatment and control groups. Surveys were not conducted on those participants to which exclusion criteria had been applied. With the exception of those victims who had refused to engage and declined further contact at the treatment stage, no further exclusions were applied.

The final survey response rates stood at 37.6% for the control group in Block A and 33.2% for the treatment group. For Block B, responses were slightly higher at 36.4% for the control group and 44.4% for the treatment group. The main reason for follow-up loss is recorded as 'unsuccessful contact' so it appears likely that the issue around caller identity may again have been a factor. The table below describes the completion rates when set against the totals per group in each block. Additionally, the number of those in the treatment group who received the treatment <u>and</u> completed a survey is outlined.

	Block A (Vehicle Crime)	Block B (Cycle Theft)
Control group + survey	147	102
completed	37.6%	36.4%
Treatment group + survey	123	126
completed	33.2%	44.4%
Treatment group +	75	86
treatment administered +	20.2%	30.2%
survey completed		

TABLE 3: FOLLOW-UP SURVEY COMPLETION RATES

Recruitment

As described in the methods section, the dates during which crimes were eligible for treatment ran from 8th August 2020 to 17th October 2020. Intervention took place around 7 to 14 days after the date the crime was recorded. The follow-up survey process began on 26th September and took place only when interventions were complete for any given week. The final surveys were completed on 19th November 2020 and analysis took place after this date.

Reason for stopped trial

The recruitment of eligible crimes stopped on 17th October due to operational reasons (see 'changes to trial design' above for full details) but sufficient base levels had been reached by this point to allow analysis to be conducted (Hinkle et al., 2013). This stoppage was planned and there was no unforeseen truncation of the trial other than for the reasons outlined above.

Baseline data

Demographic data

Data around the demographic profiles of those involved in the trial was collected at various points. Some data – gender, age and repeat victimization status – was collected by the MPS at the time the crime was recorded. These data can provide a useful comparator when considering whether the completers are similar to the non-completers, and whether the random assignment generated balanced groups within each block.

Gender

Whilst the overall populations of the Boroughs where the trial took place had a gender split approximating 50:50, the same could not be said about either the population for the trial as a whole or for the population of each block. In both cases male victims were disproportionately represented. The proportions seemed to reflect the increased likelihood of victimisation for male victims as seen in most crime types (ONS, 2017). Interestingly, despite not reflecting the overall population of the Boroughs, the gender split in both blocks were very similar to each other – as illustrated by the table below. TABLE 4: GENDER SPLITS - BLOCK A AND B

	Male n (%)	Female n (%)	Not stated n (%)
Block A	169 (62.6%)	99 (36.6%)	2 (0.7%)
Block B	145 (63.6%)	79 (34.6%)	4 (1.8%)

Within the two Blocks, the gender ratio breakdown was as follows: The Block A control group had 60% (n=88) male victims, 39% (n=57) female victims and 1% (n=2) declined to state their gender. For the treatment group there was a slightly higher proportion of male victims at 66% (n=81) whilst the remaining 34% (n=42) were female. No-one in the treatment group declined to state their gender. In Block B, the control group had 62% (n=63) males, 36% (n=37) females and 2% (n=2) declined to state their gender their gender. For the treatment group, again the proportion of male victims was slightly higher than the control group, with 65% (n=82) victims being male. Females accounted for 33% (n=42) of victims and, again, 2% (n=2) declined to state their gender.

Thus, the proportions within the treatment and control groups broadly reflected the overall gender split for the trial, however it is clear that victimisation in all cases was concentrated towards male victims.

FIGURE 3: GENDER RATIOS BLOCK A VS BLOCK B



Age

For the purpose of this trial, victims in Blocks A and B were divided into age-groups for comparative sub-group analysis. These groups were – victims 24 and under, 25-34, 35-44, 45-54, 55 and over. Any smaller grouping led to sample sizes which would have resulted in unstable modelling when analysed.

In terms of age spread, it is also useful to review the mean age (and standard deviation) of each Block in order to understand differences between the demographics of victims based on the crime type. Victims in the trial population as a whole were weighted towards the mid-range, with a mean age of 39 years (sd=14.47). However, when exclusions had been applied and the population was broken down into the blocks, Block A had a slightly higher proportion of older victims, with a mean age of 44 years (sd=14.73), whilst Block B had a higher proportion of younger victims, with a mean age of 35 years (sd=11.08). Given the nature of the crime types, this difference in age spread between the blocks is not unsurprising. The figure below illustrates this by comparing ages for each block – with control and treatment groups shown above each other.

FIGURE 4: AGE SPREAD: BLOCK A VS BLOCK B



Repeat Victimisation

The third demographic that was analysed related to those victims who stated that they had been repeat victims of crime. It should be noted, however, that this question was asked of victims at the time of reporting their crime and was specific in that it related to crimes that had occurred within the previous 12 months.

Levels of repeat-victimisation in Block A and Block B were very similar to the overall trial population – as shown in the pie charts below. In Block A 13.7% (n=37) of victims stated that they had been a victim of crime in the preceding 12 months compared to 12.7% (n=29) in Block B. With an overall crime rate in both Boroughs of approximately 121 crimes per 1000 people (Greater.London.Authority, 2020) these levels of repeat victimisation would appear to be broadly in proportion to those that would be expected (+/-5%).

In terms of the breakdown of the treatment and control groups, there were some slight differences from the overall population of the Blocks – in the Block A control group, proportions of people who stated they were repeat victims or who did not provide an answer were slightly higher (16% (n=24) for repeat victims and 12% (n=12) for not stated) than for the population of the block and the proportion who stated they had not been a victim of crime in the preceding 12 months was correspondingly lower – at 72% (n=106). In Block A's treatment group, however, the converse was true in that the proportion of people who said they were repeat victims was lower – at 11% (n=13) and very much lower in terms of those who did not provide an answer which stood at 5% (n=6). Similarly, numbers who said they were not repeat victims were higher to offset this, standing at 85% (n=85) of the group.

For Block B, in both the treatment and control groups, proportions were identical to the overall population of the Block as a whole. In the control group the percentages were identical to the overall population, with 12.7% (n=13) stating they had been repeat victims. The same figure was seen in the treatment group – 12.7% (n=16) as well. Proportions of those saying they were first time victims

were also identical – with 76% in both treatment and control groups (Block A n=78, Block B n=96) and the same proportions were seen in those who did not provide an answer to this question, at 11% (Block A n=11, Block B n=14).

The figure below illustrates the proportions of repeat victimisation, with the first chart showing the total trial population (n=1694) and then subsequent charts showing the Block breakdown and proportions in both treatment and control groups. This illustrates that the randomisation process generated treatment and control groups that were relatively balanced when compared against repeat victimisation levels for the overall trial population.

FIGURE 5: REPEAT VICTIMISATION LEVELS: BLOCK A VS BLOCK B



Self-defined ethnicity

The MPS does not mandate the recording of data in respect of self-defined ethnicity at the time of crime reporting and therefore the data for this sub-group analysis is purely drawn from survey

responses. As such, there is no overall comparison data for the trial population as a whole in respect of self-defined ethnicity.

Whilst the questionnaire asked victims to define their ethnicity based on the 18 point scale (see **Appendix (E)**) used in census data (ONS, 2018b) , the analysis looked at comparing victims who defined themselves as White (White British, White Irish, White Other) against all other ethnic groups – referred to here as Black and Minority Ethnic (BAME) groups. There is a risk in oversimplifying this data in this way as it may fail to account for differences within BAME communities (Barrett et al., 2014). However, given the study's sample size this grouping was necessary in order to allow for effective analysis to take place.

Broad analysis of the proportions of White vs BAME victims show that there was victimisation disproportionality in terms of race. In both Block A and B people who self-defined as 'white' were slightly more likely to be victims than the proportion of the overall census population figures on CN BCU (see Table 1) would suggest (ONS, 2018b). For Block A, white victims made up 61% (n=164), BAME victims 30% (n=82) whilst 9% (n=24) declined to define their ethnicity. This disproportionality was more evident in Block B, where 73% (n=167) defined themselves as white, compared to 20% (n=45) BAME and 7% (n=16) declined to define their ethnicity.

Further analysis was done in respect of the treatment and control groups themselves – For Block A, white victims accounted for 64% (n=94) of the control group and 57% (n=70) of the treatment group. BAME victims accounted for 27% (n=39) of the control group and 35% (n=43) of the treatment group. Those who did not state their ethnicity accounted for 10% (n=14) of the control group and 8% (n=9) of the treatment group. These results meant that the ethnic breakdown for both treatment and control groups for Block A was broadly in line (+/-5%) with the proportions of the block as a whole.

There was a similar picture in Block B – where white victims accounted for 71% (n=72) of the control group and 75% (n=95) of the treatment group. BAME victims accounted for 17% (n=17) of the control group and 22% (n=28) of the treatment group. There was a slight difference, however, in

respect of those for whom ethnicity was not recorded, where the results were weighted towards the control group. This accounted for 13% (n=13) of the group, set against only 2% (n=3) of the treatment group. However, for both white and BAME victims, the proportions were again broadly in line (+/-5%) with the proportions of the overall Block.



FIGURE 6: SELF-DEFINED ETHNICITY: BLOCK A VS BLOCK B

48

White BAME = Not stated/known

White BAME Not stated/known

Outcomes and Estimation

Comparison of treatment and control groups took place with the tests as outlined in the methods section Table 5 shows results of the descriptive analyses and outlines the mean values of the responses, together with their standard deviations. It is important to note that the responses for questions 1-6 were based on a 7-point Likert Scale, whilst those for questions 7-9 were based on a 3-point scale.

	Block A (V	ehicle Theft)	Block B ((Cycle Theft)
	Control	Treatment	Control	Treatment
Question 1 (Reporting process)	4.578 (1.547)	4.333 (1.581)	4.225 (1.468)	4.468 (1.483)
Question 2 (Follow-up actions)	4.415 (1.577)	4.252 (1.539)	3.961 (1.425)	4.317 (1.572)
Question 3 (Kept informed)	4.456	4.455	4.235	4.667
	(1.634)	(1.477)	(1.422)	(1.392)
Question 4 (Kept updated)	4.442	4.455	4.206	4.817
	(1.642)	(1.483)	(1.43)	(1.394)
Question 5 (Treatment)	5.054	4.821	4.48	5.127
	(1.369)	(1.465)	(1.487)	(1.368)
Question 6 (Overall service)	4.66	4.455	4.196	4.754
	(1.576)	(1.558)	(1.547)	(1.462)
Question 7 (Met expectations)	1.762	1.74	1.667	1.849
	(0.844)	(0.636)	(0.758)	(0.618)
Question 8 (Prior opinion of police)	2.361	2.341	2,235	2.484
	(0.737)	(0.795)	(0.854)	(0.663)
Question 9 (Did your opinion change as	2.02	1.967	1.941	2.079
a result of this incident)	(0.473)	(0.54)	(0.539)	(0.543)

 TABLE 5: MEANS (AND STANDARD DEVIATIONS) FROM QUESTIONNAIRE RESPONSES

Following these initial calculations, it was then possible to conduct calculations for statistical significance (Two tailed T-Test) and for effect size (Cohen's *d*). These tests were applied on a question by question basis and across both blocks. Because each question explored different aspects of satisfaction, results were not aggregated. The results of these calculations are outlined overleaf.

	Blo	ck A	Blo	ck B
	Effect Size (d)	90% CI	Effect Size (d)	90% CI
Question 1	-0.157	[-0.357 , 0.045]	0.191	[-0.055 , 0.384]
(Reporting process)				
Question 2 (Follow-	-0.103	[-0.305 <i>,</i> 0.097]	0.238*	[0.016 , 0.456]
up actions)				
Question 3 (Kept	0.000	[-0.201 , 0.201]	0.307**	[0.086 , 0.527]
informed)				
Question 4 (Kept	0.008	[-0.193 , 0.209]	0.433***	[0.212 , 0.655]
updated)				
Question 5	-0.165	[-0.366 , 0.036]	0.453**	[0.232 , 0.676]
(Treatment)				
Question 6 (Overall	-0.131	[-0.332 , 0.071]	0.371***	[0.150 , 0.592]
service)				
Question 7 (Met	-0.030	[-0.230 , 0.172]	0.264*	[0.046 , 0.486]
expectations)				
Question 8 (Prior	-0.025	[-0.226 , 0.176]	0.325**	[0.109 , 0.550]
opinion of police)				
Question 9 (Did	-0.104	[-0.306 , 0.096]	0.255*	[0.035 , 0.475]
opinion change)				

TABLE 6: EFFECT SIZES WITH CONFIDENCE INTERVALS

* p < 0.10; **p < 0.05; *** p < .01

These results show that in respect of Block A there was a propensity towards negative, or at best negligible, results in respect of the treatment. It should be noted that none of these results were statistically significant. However the converse is true in respect of Block B. With the exception of Question 1, all of the interventions were statistically significant at P<0.1 and they were significant at P<0.05 in respect of Questions 3,4,5,6 and 8 (see **Appendix (C)**). Coupled with this is the fact that, again with the exception of Question 1, there was a small (d=0.2-0.5) effect size seen in responses to all other questions.

When the effect size is shown in terms of a standardized relative difference using the equation ((Control/Treatment)/Control), the responses to the treatments between blocks A and B can be plotted for comparison in terms of a percentage difference between treatment and control group responses. Thus, 0% means the treatment responses were no different to the control responses, whilst a positive difference illustrates a beneficial effect in terms of a percentage improvement and vice versa

for negative effects. Figures 7 and 8 illustrate this relative difference on forest plots with the 90% confidence interval shown.





FIGURE 8: RELATIVE DIFFERENCE OF EFFECT SIZE - BLOCK B



Results of other analyses performed

Four categories of comparative sub-group analysis were pre-planned and included in this trial – these aimed to compare the impact of the treatment on satisfaction levels when considering gender, age, repeat victimisation and self-defined ethnicity. Additional data was also collected during the survey process in respect of victim occupation and victim-perceived vulnerabilities, however analysis was not conducted in respect of these last two sub-groups for a variety of reasons, but principally because they had not been included at the planning stages of the trial.

The analysis conducted in respect of these sub-groups mirrored that of the main analysis process – in that descriptive analysis in respect of numbers, means and standard deviations was obtained. Tables of results for the mean responses and standard deviations in respect of each sub-group can be found at **Appendix (D)**. Following collation of this data, it was then possible to perform calculations to assess statistical significance and effect size in respect of the sub-groups. As per the main analysis, statistical significance was measured at P<0.1. When considering the results of each of the sub-group analyses, there are two caveats that need to be noted:

Firstly the sub-groups were not, in themselves, randomised. The populations of the subgroups formed part of the overall population of each Block which had been randomised as part of the main trial but no further randomization was conducted after this point. Therefore there is a risk that some bias may be present in these results and they therefore naturally rank lower in terms of measures of validity such as the Maryland scale (Sherman, 2019, Sherman et al., 1998). Secondly, in some of the sub-groups the sample size was low. This is particularly the case in respect of some of the age groups, but is also true in that n<20 in other groups of the Blocks, such as repeat victims and BAME victims. Because of these low sample sizes, some statistical modelling was unstable. Where the sample (n) was below 20 in either a treatment or control group, alternative calculations for effect size (Hedge's g) were conducted and these results are included alongside the Cohen's *d* result.

TABLE 7: SAMPLE SIZES OF SUB-GROUPS FOR ANALYSIS

		Block A Control (n)	Block A Treatment (n)	Block B Control (n)	Block B Treatment (n)
Gender	Male	87	79	63	82
	Female	57	42	37	28
Age	24 and under	9*	8*	13*	18*
	25-34 years	37	30	46	58
	35-44 years	32	32	19*	31
	45-54 years	25	26	15*	10*
	55 and over	42	23	8*	7*
Repeat	Repeat victim	24	13*	13*	16*
victimization	1 st time victim	105	103	78	95
Self-defined	White	94	70	72	94
ethnicity	BAME	39	43	17*	28

Where results are marked * - Hedge's G is included in any effect size reporting

Gender

Comparative analysis was conducted between male and female victim groups. Victims who declined to state their gender were not included in this sub-group analysis although their valid survey responses form part of other results.

From the sample sizes, mean responses and standard deviation data in respect of gender it was then possible to run calculations in respect of effect size and statistical significance (p<0.1). Again these were broken down by block. The results of these calculations showed that, like the trial the majority of positive treatment effects were mostly seen in Block B. However, the calculations also showed that the positive effects were almost wholly seen in the responses given by male victims. In some cases the effect size was notable – with medium effect sizes (d>0.5) seen by male victims of cycle theft in how they felt they were kept informed and treated. Conversely little in the way of positive effects were seen in regard to female victims in either block. Any effects on female victims were, at best, small and none were statistically significant even at p<0.1.

		Block A -	- Vehicle cri	me		Block B - Cycle theft			
	Male victims		Femal	e victims	Male victims		Fema	le victims	
	Significance (p)	Effect size (d)	Significance (p)	Effect size (d)	Significance (p)	Effect size (d)	Significance (p)	Effect size (d)	
Q1: Time of reporting	0.467	-0.144	0.460	-0.149	0.158	0.240	0.909	0.026	
Q2: Follow-up actions	0.733	0.290	0.474	-0.145	0.040**	0.350	0.555	0.137	
Q3: Kept updated	0.575	0.354	0.579	-0.112	0.032**	0.367	0.684	0.093	
Q4: Kept informed	0.674	0,363	0.812	-0.048	0.002**	0.533	0.418	0.186	
Q5: Treatment	0.352	0.061	0.306	-0.209	0.002**	0.546	0.371	0.207	
Q6: Overall service	0.256	0.191	0.888	-0.028	0.008**	0.455	0.684	0.094	
Q7: Met expectations	0.106	-0.257	0.912	0.023	0.369	0.160	0.111	0.377	
Q8: Prior opinion	0.595	0.149	0.617	-0.104	0.322	0.173	0.507	0.155	
Q9: Opinion now	0.615	-0.080	0.967	-0.008	0.086*	0.292	0.317	0.231	

TABLE 8: STATISTICAL SIGNIFICANCE AND EFFECT SIZES OF TREATMENT BY GENDER

*P < 0.1; **P<0.05

One result worth noting is in respect of how male victims of vehicle crime felt the service met their expectations – where a 'backfiring' effect was seen in terms of effect size (d=-0.257). Smaller negative results were also seen in response to a number of the questions from female victims of vehicle crime as well. Whilst none of these results were statistically significant and may have occurred by chance, this appeared to be a noticeable trend in respect of the experience of vehicle crime victims of both genders.

The analysis shows that there appear to be differences in how men and women perceived the treatment, both in terms of the impact and effect of the treatment on their satisfaction. Whilst male victims of cycle theft clearly responded to the treatment given with an appreciable effect on their satisfaction, this was not replicated in respect of female victims. In terms of victims of vehicle crime, whilst the treatment itself did not appear to be significant, in some instances adverse effects were

noted between treatment and control groups –particularly among female victims where there seemed to be a broadly negative effect.

Age

For the purpose of assessing whether effects were concentrated by age, the surveyed population of each block was divided into age groups: Victims 24 and under, 25-34, 35-44, 45-54, 55 & over. The age groups were divided in this way in order to enable a measurable sample size in each group to be assessed, but it should be noted that even with this subdivision, there were some areas where sample sizes were small – particularly among young (under 25) victims of vehicle crime and older (55 and over) victims of cycle crime. These small sample sizes were unsurprising given the crime types – where younger people may be less likely to own or use a vehicle in central London and, conversely, older people are less likely to use cycles as a form of conveyance when compared to younger people. What these small sample sizes mean in practice, however, is that for these particular groups caution should be used when interpreting the results. In cases where n<20 for a given group, Hedge's *g* was also calculated to allow accuracy in terms of effect size. Whilst Cohen's *d* is shown for consistency, where relevant the 'g' result is also shown.

As before, descriptive analysis was conducted to identify sample size, mean responses and standard deviations allowing calculations of significance and effect size to be conducted. When the mean values were assessed for both blocks, it became apparent that there were substantial and differing trends in terms of satisfaction patterns in the age groups at the peripheries – For younger age groups in respect of vehicle crime there appeared to be a backfiring effect, where the treatment appears to have led to negative results. Conversely the treatment appeared to have positive effects for older victims of vehicle crime but a negative effect was seen in older victims of cycle theft. In some of these cases the effect sizes were large, but for both young victims and older victims, the sample sizes were low and because of this the likelihood that outlying responses may have affected the results cannot be discounted. In order to maintain clarity, the summaries of effect size and significance have

been divided into two tables by block. Given the small sample sizes in some instances, Hedge's g is also shown where n<20 for either the treatment or control group in response to any given question.

	24 and under		25	5-34	3	35-44 45-54			55 and over	
	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)
Q1	0.225	d=-0.659 g=-0.652	0.608	d=-0.128	0.193	d=-0.334	0.396	d=-0.244	0.176	d=0.351
Q2	0.225	d=-0.973 g=-0.984	0.849	d=-0.047	0.594	d=-0.316	0.581	d=-0.159	0.453	d=0.195
Q3	0.152	d=-0.801 g=-0.810	0.700	d=0.094	0.686	d=-0.103	0.941	d=0.021	0.279	d=0.283
Q4	0.483	d=-0.374 g=-0.376	0.788	d=0.086	0.325	d=-0.252	0.820	d=-0.065	0.050*	d=0.502
Q5	0.394	d=-0.467 g=-0.376	0.399	d=-0.212	0.127	d=-0.394	0.755	d=-0.090	0.453	d=0.187
Qe	0.057*	d=-1.070 g=-1.071	0.465	d=-0.181	0.730	d=-0.088	0.679	d=-0.119	0.494	d=0.176
Q7	0.023**	d=-1.361 g=-1.361	0.046**	d=-0.523	0.555	d=-0.156	0.749	d=-0.093	0.949	d=-0.016
Q8	0.109	d=-0.966 g=0.969	0.275	d=0.280	0.211	d=0.335	0.534	d=-0.181	0.791	d=-0.070
Q9	n/a	d=0.000 g=0.000	0.827	d=-0.056	0.799	d=0.065	0.156	d=-0.417	0.444	d=0.237

TABLE 9: STATISTICAL SIGNIFICANCE AND EFFECT SIZES OF TREATMENT BY AGE GROUP - BLOCK A

*P<0.1; **P<0.05

	24 an	24 and under		5-34	35	5-44	1	15-54	55 and over	
	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)
Q1	0.118	d=0.612 g=0.614	0.681	d=0.082	0.021**	d=0.702 g=0.691	0.922	d=-0.044 g=-0.044	0.527	d=-0.355 g=-0.345
Q2	0.150	d=0.573 g=0.584	0.166	d=0.276	0.054*	d=0.595 g=0.597	0.832	d=-0.092 g=-0.093	0.799	d=0.146 g=0.147
Q3	0.044**	d=0.836 g=0.863	0.223	d=0.241	0.074*	d=0.569 g=0.588	0.922	d=-0.043 g=-0.044	0.100	d=0.000 g=0.000
Q4	0.006**	d=1.196 g=1.235	0.176	d=0.269	0.009**	d=0.863 g=0.898	0.814	d=0.104 g=0.106	0.799	d=-0.146 g=-0.147
Q5	0.024**	d=0.929 g=0.946	0.079*	d=0.353	0.033**	d=0.673 g=0.685	0.771	d=0.129 g=0.132	0.541	d=-0.352 g=-0.354
Q6	0.058*	d=0.757 g=0.763	0.254	d=0.230	0.005**	d=0.881 g=0.877	0.776	d=-0.126 g=-0.129	0.853	d=-0.104 g=-0.103
Q7	0.110	d=0.639 g=0.645	0.352	d=0.191	0.089*	d=0.543 g=0.564	0.836	d=0.091 g=0.092	0.404	d=-0.514 g=-0.497
Q8	0.024**	d=0.988 g=0.993	0.213	d=0.256	0.368	d=0.264 g=0.254	0.596	d=-0.247 g=-0.254	0.834	d=-0.117 g=-0.116
Q9	0.123	d=0.593 g=0.587	0.460	d=0.149	0.021**	d=0.718 g=0.720	0.906	d=0.052 g=0.026	0.737	d=-0.191 g=-0.190

TABLE 10: STATISTICAL SIGNIFICANCE AND EFFECT SIZES OF TREATMENT BY AGE GROUP - BLOCK B

*P < 0.1; **P<0.05

Examining the responses in terms of significance and effect size, there appear to be a number of statistically significant responses – particularly in Block B - and in some of these cases the effect size is large (d>0.8). The backfiring effect mentioned above is particularly noticeable, albeit not statistically significant, among young victims (under 25) of vehicle crime. This effect is countered by large and statistically significant positive treatment effects on this same age group in respect of cycle theft. This positive impact is also seen in cycle theft victims in the 35-44 age bracket. Finally, some positive impacts were seen in older victims of vehicle crime, which appear to buck the trend seen in other subgroups where vehicle crime victims experienced negligible or negative effects from the treatment. All of these findings, however, should be tempered with caution as the sample sizes in each group were low enough that outliers may have affected the results. It appears however, from these results, that age is likely to have an impact in terms of effect concentration and this may warrant further study.

Repeat victimisation

Police have historically tailored their victim support processes towards repeat victims of crime in efforts to prevent further offences and reduce the harm caused to victims (Farrell and Pease, 1993, Pease and Farrell, 2016). Indeed, the MPS requires officers when reporting crimes to ask victims if they have been victimised in the previous 12 months and, if so, to consider whether extra victim support is needed. In order to ascertain whether the treatment in the trial provided support to repeat victims, analysis was conducted to compare the satisfaction of those who had been victims of crime in the year prior to reporting against those who had not been or who had not specifically stated they had. It should therefore be noted that when the results refer to repeat or non-repeat victims, this only relates to the preceding 12 months and is based on self-declaration by the victims themselves. It should also be noted that at the time of reporting victims are not asked the nature of the crime they have suffered previously and so this data was not available for the trial. Therefore it is not known whether there is a relationship between any particular reported crime types and subsequent impacts on victim responses to the treatment or the questionnaire.

This analysis again took the form of the comparisons seen in other sub-groups and the same caveats and cautions should apply – particularly as the sample sizes of those who stated they had been repeat victims were low. As before, calculations were conducted to ascertain levels of statistical significance and effect size in terms of repeat victims vs non-repeat victims. Because of small sample sizes in respect of repeat victims, calculations for Hedge's g were also conducted when either the treatment or control group n<20. The results of these calculations indicated that the treatment had some small and statistically significant effects on non-repeat victims of cycle theft in most areas questioned. However in respect of non-repeat victims of vehicle crime, again a backfiring effect of negative responses was seen. This was smaller than in other sub-groups, nor were the responses statistically significant, except in respect of how these victims felt they had been treated.

For repeat victims, it appears that the treatment had little or negligible effects on their satisfaction – some small effects were seen in respect of victims of cycle theft and the negative effects for victims of vehicle crime were not seen, but in neither case were the majority of responses statistically significant. The same caveat of small sample size remains, however, and so caution is urged before drawing wider conclusions from these results.

		Block A -	Vehicle crim	ne	Block B – Cycle theft					
	Repe	at victims	Non-rep	eat victims	Repe	at victims	Non-rep	eat victims		
	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)	Significance (p)	Effect size (d/g)		
Q1: Time of reporting	0.738	d=0.119 g=0.118	0.196	d=-0.181	0.516	d=-0.253 g=-0.250	0.103	d=0.253		
Q2: Follow-up actions	0.826	d=0.077 g=0.075	0.226	d=-0.169	0.677	d=0.162 g=0.161	0.106	d=0.263		
Q3: Kept updated	0.448	d=0.260 g=0.250	0.308	d=0.145	0.720	d=0.140 g=0.139	0.036**	d=0.330		
Q4: Kept informed	0.328	d=0.335 g=0.319	0.751	d=-0.044	0.349	d=0.368 g=0.367	0.013**	d=0.388		
Q5: Treatment	0.651	d=0.159 g=0.156	0.047**	d=-0.279	0.608	d=0.201 g=0.202	0.001**	d=0.522		
Q6: Overall service	0.936	d=-0.029 g=-0.029	0.189	d=-0.205	0.733	d=0.137 g=0.137	0.023**	d=0.362		
Q7: Met expectations	0.303	d=0.389 g=0.383	0.051*	d=0.280	0.786	d=0.111 g=0.112	0.074*	d=0.293		
Q8: Prior opinion	0.541	d=0.229 g=0.235	0.961	d=0.007	0.888	d=0.057 g=0.056	0.108	d=0.271		
Q9: Opinion now	0.168	d=-0.501 g=-0.485	0.658	d=-0.062	0.858	d=-0.073 g=-0.075	0.042**	d=0.315		

TABLE: STATISTICAL SIGNIFICANCE AND EFFECT SIZES OF TREATMENT ON REPEAT VICTIMS

*P < 0.1; **P<0.05

Race

At the time of reporting their crime victims were not required to define their ethnicity and therefore this was the only demographic characteristic for which overall data for the entire trial population (whether surveyed or not) was not available. Victims who were surveyed were asked to define their ethnicity as per the 18 point scale used in respect of census data (see **Appendix (E)**) (ONS, 2018b). The results from this show the ethnic breakdown for each block to be as follows:

TABLE 11: SELF-DEFINED ETHNICITY BY BLOCK

	Block A – Vehicle crime		Block B – Cycle theft		
	Control	Treatment	Control	Treatment	
A1 – Indian	1	2	2	2	
A2- Pakistani	1	0	0	0.	
A3- Bangladeshi	4	6	0	0	
A9 - Any other Asian	8	6	2	4	
B1 – Caribbean	5	4	0	2	
B2 – African	10	11	0	6	
89 - Any other Black	2	0	1	1	
M1 – White & Black Caribbean	2	1	3	0	
M2 – White & Black African	1	1	Ō	0	
M3 – White & Asian	1	4	1	2	
M9 - Any other mixed	2	2	4	6	
N3 — Did not understand	1	1	0	0	
N4 – Declined to define	13	9	13	3	
01 - Chinese	0	3	3	1	
O2 — Any other ethnicity	2	3	1	4	
W1 - White British	70	47	34	65	
W2 – White Irish	4	2	4	0	
W3 - Any other White	20	21	34	30	

Having broken down the numbers into these 18 categories, it became clear that comparative analysis would be impossible with such small sample sizes. It was therefore decided to aggregate racial groups together into two categories for the purpose of analysis – with all victims from groups W1,W2,W3 in the 'White' and all other ethnicities in the 'BAME' group. It is appreciated that by dividing the analysis into two broad groups in this way runs a risk of oversimplifying issues that may exist between ethnic groups (Barrett et al., 2014), but given the fact that in London there is a known satisfaction gap between white and BAME victims (MOPAC, 2020b) analysis of results divided in this fashion can help to inform future policy direction. Responses from victims who did not understand the question or who declined to define their ethnicity were not counted as part of the comparative analysis for this sub-group but their responses do feature in the overall analysis and, where relevant, as part of other sub-group analyses.

1	1	B	lock A			Block B			
	Whit	e victims	BAN	ME victims	White	victims	BAM	Evictims	
	Significance (p)	Effect size (d/g)							
Q1: Time of reporting	0.164	d=-0.221	0.404	d=0.187	0.438	d=0.123	0.242	d=0.378 g=0.379	
Q2: Follow-up actions	0.266	d=-0.177	0.461	d=0.165	0.215	d=0.196	0.110	d=0.522 g=0.526	
Q3: Kept updated	0.803	d=-0.039	0.166	d=0.313	0.182	d=0.120	0.060*	d=0.638 g=0.658	
Q4: Kept informed	0.663	d=-0.069	0.130	d=0.343	0.034**	d=0.337	0.091*	d=0.558 g=0.560	
Q5: Treatment	0.148	d=-0.233	0.543	d=0.136	0.054*	d=0.305	0.142	d=0.489 g=0.502	
Q6: Overall service	0.253	d=-0.182	0.246	d=0.261	0.197	d=0.205	0.022**	d=0.752 g=0.747	
Q7: Met expectations	0.157	d=-0.225	0.275	d=0.255	0.547	d=0.098	0.137	d=0.486 g=0.481	
Q8: Prior opinion	0.866	d=0.027	0.717	d=0.083	0.111	d=0.256	0.962	d=-0.016 g=-0.017	
Q9: Opinion now	0.536	d=-0.100	0.455	d=0.167	0.354	d=0.147	0.114	d=0.501 g=0.499	

TABLE 12: STATISTICAL SIGNIFICANCE AND EFFECT SIZES OF TREATMENT BY SELF-DEFINED ETHNICITY

*P<0.1; **P<0.05

From the analysis of effect size and significance, some interesting results were seen. The results show that for BAME victims, the treatment may have had an effect in improving satisfaction. Small positive effects, although not statistically significant, were seen in BAME victims of vehicle crime. This differs from the experience of White victims of vehicle crime, where the negative 'backfire effect' seen in other sub-groups was also present. However, for BAME victims of cycle theft, the treatment appears to have had positive results. Statistically significant results were seen in respect of how BAME victims felt they were kept informed, updated and how satisfied they were with the overall service they received. Perhaps more importantly, however, was the magnitude of the effect sizes seen – where medium effects (*d*>0.5) were seen in most areas, set against smaller effects felt by White victims of cycle theft.

These data suggest there may be a concentration of positive effects in terms of BAME victims – potentially allowing for an opportunity to close the BAME satisfaction gap. However, these results need to be tempered with the same caution that applies to other sub-groups – in that small sample sizes and a small possibility of bias may have affected these results.

Chapter 4: Discussion of results

This chapter will examine the overall results of the trial and pose potential explanations for the results seen. As an adjunct to these main findings, results of the sub-group analyses will also be examined with respect to the effects that were observed and again consider potential explanations for these concentrations of effects. The trial methodology will be reviewed, outlining key strengths and limitations of the methods used. Recommendations will also be made for further research in respect of the findings of the trial. The operational implications for police agencies will be considered. These include considerations for implementation and targeting of treatment based on crime and victim type. Cost implications are also considered and, finally, recommendations are made in respect of strategic issues for police leaders to address when considering implementation of a victim call-back programme in their agencies.

Discussion of overall results

The results show that the intervention of a reassurance call-back to victims of crime can indeed have an effect on victim satisfaction, but it is clear that the nature and size of the effect differs depending on the crime type and victim characteristics. This supports the arguments made by Kumar (Kumar, 2018) in that the type of the crime reported may have a direct influence on the levels of satisfaction felt by victims of crime.

In respect of the victims of cycle theft the call-back treatment appeared to have broadly positive effects that were statistically significant in answer to most of the survey questions. In relation to some of the questions – particularly those relating to how victims were kept updated and treated,

the effect size meant around a 15% improvement on responses from the control group – hardly an insignificant improvement. However, the same could not be said for victims of vehicle crime, where effects appeared negligible at best and negative in some cases – albeit none of these results were statistically significant even at the p>0.1 level.

An explanation for this appreciable difference between the two groups lies in potentially differing expectation levels between them. When considering the impact of victim expectations, Chandek concludes that:

"Expectations play a vital role in determining crime victim satisfaction with the police." (Chandek and Porter, 1998)

It appears likely that victims of vehicle crime may have had relatively higher expectation levels that police would progress the investigation to a greater degree – possibly due to the higher monetary values involved and perceptions that police have, in previous years, treated car crime as a priority target (Davenport, 2012). A 1991 study of victim satisfaction noted that follow-up contact by police needed to be perceived by victims as being of some relevance or legitimacy to them, otherwise there was little impact on satisfaction levels (Brandl and Horvath, 1991). In the case of this trial, if victims of vehicle crime expected that their case was going to be progressed by police, a follow-up call to simply offer reassurance and reaffirm that the crime had been screened out was likely to be of little consequence to them and potentially may even explain the 'backfiring' effect where negative impacts were seen.

This explanation also appears to be valid when considering the expectations and experiences of cycle owners, where up to 39% of cyclists believed their cycle was likely to get stolen regardless of the efforts they made to secure it (Bryan-Brown and Savill, 1997). With public acknowledgements from some police forces that investigating cycle theft is a low priority (Hellon, 2021), it is reasonable to suppose that <u>any</u> subsequent contact from police to victims of cycle theft has the potential to exceed their initially low expectations of the police with respect to their crime. It has already been

established from previous experience of victims of volume crime in the MPS that the more that victim expectations are met, then the more likely it will be that they will express satisfaction with the police (Aihio, 2017). This therefore appears to be a plausible explanation for the positive effects the treatment had on victims of cycle theft.

When examining the differences between the two groups, it is also worthwhile briefly noting that the observed differences were also present in respect of two of specific questions in the survey – Question 1 and Question 8. Question 1 asked respondents to rate their satisfaction in respect of the reporting process and Question 8 attempted to gauge respondents' opinions of police prior to the incident. Logic would dictate that the treatment should not have influenced responses to either question. However, it is clear that the same differences seen in other responses are still present - with cycle theft victims indicating positive effects and vehicle crime victims indicating that the call-back may have had a wider effect on all responses given, regardless as to whether the question directly related to the treatment given.

Finally, when considering responses to the last survey question – "As a result of this incident, has your opinion of the police got better, got worse or not changed?" – there was a positive effect for victims of cycle theft. Equally, for victims of vehicle crime, although there was a slight negative effect, this was less so than for some other questions. This seems to contradict the theory that a single encounter or experience from victims may not influence their wider opinions of policing (Reisig and Chandek, 2001) and thus shows that there may be wider benefits for policing through conducting interventions of this type.

Discussion of sub-group results

When considering sub-groups, the trends broadly reflected the results from the overall blocks – but there were some cases of concentration of effect. Whilst small sample sizes in some cases mean the results should be treated with caution. Some results are suggestive of potentially powerful impacts. Because of the small sample sizes, however, it is recommended that further research is conducted in respect of sub-groups in order to more accurately assess the impacts of the treatment on these victims.

Gender

The treatment appeared to have a negligible effect for female victims in both blocks and none of the responses were statistically significant. Also absent for female victims, however, were any sizeable negative impacts of the treatment. Whilst female victims only accounted for a third of the population, it appears that the treatment effects were seen almost wholly in male victims. This appears to contradict the earlier assertion that female victims were likely to respond favourably to reassurance contact (Foley & Terrill, 2008). The theory that differing expectation levels may have led to this result is also difficult to argue in this instance – almost all criminological research around victimisation and gender is based around topics such as sexual offences and domestic abuse and there is little in previous studies or in the survey responses that offers a credible explanation as to why the treatment effects seem to be concentrated amongst male victims. Further research may therefore be needed to explore this in more detail.

Repeat victimisation

The positive effects of the treatment were not as prominent in respect of repeat victims. Whilst some small effects on both groups were seen, neither set of results were statistically significant. This is contrasted against the experience of first-time victims – where the treatment appeared to have been positively received by victims of cycle theft. Again, the explanation of differing expectations may be valid, but it is also known that repeat victims are often less satisfied with the police response to their crimes (van Dijk, 2001) and this appears borne out by the mean levels of responses to the survey questions in respect of both treatment and control groups (see Appendix C) when contrasted with the first-time victims.

The analysis by age groups demonstrated concentrations of effect – particularly in respect of younger victims (<25 years) and older victims (>55 years). In both these groups, the observed effects were somewhat reversed – younger victims of car crime were likely to have expressed negative effects from the treatment and these were of large size, whilst young victims of cycle theft were the most positively impacted sub-group in the study. Conversely, the negative effects for older victims of car crime were largely absent but older victims of cycle theft reported a negligible or even negative effect from the treatment, a result not seen in other sub-groups.

Caution should be given to placing too much credence to these results as these groups were amongst the smallest sample sizes in the subgroup analysis. Again, however, the explanation of differing expectations is potentially valid: Owning and running a motor vehicle in London is neither cheap nor simple, and for a young person the impact of such a crime is likely to be of more significance to them personally, given lower average wages and higher insurance costs. This may lead to a greater expectation on the part of younger victims that police will support them and progress investigations. Equally, the number of older cyclists in the population was very small and a possible explanation is that those cycling at older ages are doing so because of personal enthusiasm for cycling rather than out of necessity and as such are likely to be more impacted by a theft, meaning that reassurance contact by police is not going to meet their expectations *viz a viz* a 'good result'. Clearly this is supposition and further research is needed to identify the causes of any such differences, but it should be noted the large and positive effects seen in younger victims of cycle theft seem to be in contrast with Brandl's 1991 findings that younger people are less likely to be satisfied with police and that in all cases recontact being only of benefit where serious property crime was involved (Brandl and Horvath, 1991).

Age

Race

Finally, there was an observed concentration of effect in terms of race – The positive effect of the callback on BAME victims of cycle theft was larger than that of white victims. Additionally, there were small positive effects for BAME victims of car crime - this is in contrast to the main results and the other sub-groups where negligible or negative effects were seen. These results are therefore promising in terms of reducing BAME satisfaction gaps, but as with the differences seen between genders a plausible explanation for the results may be difficult to argue. The fact that there are lower overall levels of confidence among the BAME community in London and more generally (Dai and Johnson, 2009, MOPAC, 2020b) may mean that it is possible that BAME victims of both car and cycle crime had low initial expectations of the police. Thus, a call-back could have exceeded these expectations to a greater level than in other groups regardless of crime type, but without further research it is difficult to argue this to any degree of certainty. What is clear, however, is that there is a potential opportunity for targeting efforts to improve satisfaction at BAME victims and that positive impacts may well be more significant than those seen in other demographic groups - this presents a promising potential for forces to consider when planning for ways to engage with and increase satisfaction among BAME communities. At present the MPS does not require victims to define their ethnicity at the time of reporting a crime online and as such this poses a challenge to assessing the efficacy of any future interventions. It is recommended that the MPS adapts its crime reporting processes to allow this to take place.

Strengths of the trial

The author was aligned to the command team for the BCU and was thus able to set up a team to support the research on the ground fairly easily (Neyroud, 2017). Additionally, buy-in from the remainder of the local command was an easier process to manage than might otherwise have been the case, meaning that there was consistent support for the trial throughout the time it ran on the BCU. This allowed effective implementation and regular meetings were possible with team

supervisors, meaning that most issues were resolved relatively speedily through a dedicated process that was understood by all involved.

Furthermore, by conducting an RCT as two separate blocks – in effect two separate experiments – the trial was able to demonstrate a high level of both reliability and treatment integrity (Sherman, 2019, Sherman et al., 1998) meaning that the results are likely to be generalisable in terms of policing practices. That said, this trial took place in inner-city London and the population does not necessarily reflect other locales. For this reason, it is recommended that this trial is replicated in other locations and by other policing agencies in order to assess the extent the findings have in terms of external validity (Ruane, 2005).

Finally, the use of officers who were prevented from doing their normal roles due to the COVID19 pandemic to conduct the surveys on a blind basis also helped to ensure treatment integrity further. This also meant that there was little to no impact from the trial process on operational policing other than the original call-back programme.

Limitations of the trial

There were a number of limitations to note in respect of the trial. Firstly, although the initial sample size was initially deemed adequate, the relatively high attrition rate in terms of unsuccessful call backs meant that the numbers of victims who received the treatment was lower than anticipated. This was further compounded by lower-than-expected response rates from the telephone survey process and as such the sample size analysed was smaller than predicted. Although responses in the overall study allowed a 90% level of confidence in the results, the impact of this attrition meant that sample sizes in the sub-groups analysed were on occasions so small that conclusions may not be reliable. This high rate of attrition was likely due to the fact that the MPS telephony system routinely hides its identity – showing as a 'withheld number' to recipients. It appears that a large proportion of victims may have declined to answer either the call-back or the survey because of this. Although this was anticipated at the planning stage and instructions were given to officers on how to bypass this feature, it is not known

to what extent officers complied with these instructions. It is recommended if this study is replicated that consideration is given by researchers at the planning and treatment delivery stage as to how to reduce the impact of this issue.

Secondly, although the treatment and control groups were randomised, the analysis could only examine responses from those victims who actually answered the survey questions. As such, there may be a potential for bias in terms of the results as only those who answered calls and responded favourably to the request to be surveyed could be included. Whilst examination of the overall demographic makeup of the groups shows that the respondent groups appeared to be broadly reflective of the trial population as a whole, the risk of potential bias cannot be completely ruled out. Additionally, in respect of the demographic sub-groups, no further randomisation took place after the initial process and therefore some additional but inadvertent bias may potentially exist in terms of these groups.

A further possible weakness in respect of the survey process was the fact that the people conducting the survey were themselves serving officers from the same command as those officers who had conducted the call-backs. There is therefore a potential that the results could have been affected. In order to mitigate against this the officers doing the survey were from different teams to the officers who conducted the call-backs and they themselves were blinded as to the reasons for the survey process. Whilst this was intended to reduce the risk of bias, it may not have eliminated it entirely. If this survey is to be replicated, it is recommended that, in line with CONSORT recommendations, there is a clear division made between the treatment provision and the research instrument (Schulz et al., 2010).

Finally, although the officers conducting the call-backs were briefed and given a checklist of points to cover, they were not constantly supervised whilst conducting the call-backs. Although regular meetings were held with the BCU to both give and receive feedback (Neyroud, 2019) these could only occur once interventions had taken place. As such, it is impossible to know or assess the

levels of consistency in terms of the quality of the call backs. The spreadsheets completed by these officers at the time of calling victims back do indicate some small differences in styles in terms of the information they recorded, but whether these translated into differences in quality or treatment fidelity is unknown.

Recommendations for further research

Improving satisfaction from younger and BAME victims has presented a challenge to police agencies in the past and these challenges still prove difficult to overcome (Barrett et al., 2014, Brandl and Horvath, 1991). The results of this trial show that there may be an opportunity to close this satisfaction gap as there appear to have been clear positive impacts on some victims. There are a number of areas to which further research may help to explore the extent of this concentration of effect. Firstly, it is recommended that a replication of this study with larger sample sizes may help to provide further clarity around effect sizes and the impact of the intervention on sub-groups. Equally, a replication of the study but examining diverse demographic samples will also allow for the sub-group effects which may have occurred to be examined in more detail. A key recommendation for further research is around differing crime types – it appears clear from the results that crime type influences the effect of the treatment, therefore it is likely to be of considerable interest to police agencies to understand more how crime type can affect victim satisfaction. Results from research into both demographic subgroups and crime types can then help to inform potential implementation plans for police agencies.

Funding

This research forms part of the Senior Leaders Masters Degree Apprenticeship (SLMDA) programme run by the Institute of Criminology at the University of Cambridge. The author's place on this programme was funded in full by the MPS, however the MPS did not mandate the research topic or design at any point.

Implications for police agencies

The results from the trial appear to support the expectancy disconfirmation model (Chandek and Porter, 1998, Reisig and Chandek, 2001) in that if victim perceived expectations are not met, then clearly satisfaction cannot be met either. It thus appears that there can be no 'one size fits all' approach to improving satisfaction and that any plans on the part of police agencies to do so will need to be nuanced with regard to both crime type and specific expectations of the victims of those crimes. To this end police agencies are recommended to consider commissioning research in order to gain a greater understanding of expectation levels of victims with regard to crime types. If police agencies are able to then tailor their interventions and contacts to specifically address these expectations through a clear and effective process then victim satisfaction is almost certain to improve (Aihio, 2017, Myhill and Bradford, 2012). One of the key implications of this trial, therefore, is that a carefully tailored programme that targets the most appropriate victims or crime types for intervention based on evidence of what works (Sherman, 2017) is potentially likely to elicit very effective results in terms of improving victim satisfaction.

At the same time, it is important to be conscious of the cost-effectiveness of the interventions. The cost of the trial was estimated using a maximum call length of ten minutes per victim. At 2020 rates for the MPS, this meant a maximum cost of £6.79 per call-back (MPS, 2020b). This meant that the call-backs across both treatment groups over the ten-week period would have cost £4448 if 100% of calls (n=655) had occurred. In the event, only 374 call backs took place, with a resultant cost of £2539, but note that this does not include the cost of any unsuccessful attempts to call victims. It may thus be more prudent for police agencies to predict costings for implementing a call-back programme based on a rate of 100% anticipated completion. Whilst these calculations account for costs in a purely financial sense, it may also be worthwhile for police commanders to consider costs in terms of officer time. The officers involved in the trial were employed in the BCU local resolution team – a unit whose main role is calling back victims of crime to take or clarify crime reports and therefore their time was already, in effect, paid for as part of the BCU overall budget. Over the course of the trial, the number of call-backs required averaged around 8 per day – meaning an approximate time cost of 1 hour, 20 minutes daily. This is not an inconsiderable amount of time for an individual officer or small unit, and so decisions will need to be made by commanders as to whether victim call-backs should be prioritised or, alternatively, passed out across their command to reduce the impact. The latter option may prove complicated in terms of ensuring quality so in order to realise the most effective benefits it may be necessary to introduce systems to effectively track interventions in order to assess progress and compliance (Slothower et al., 2015).

If police agencies are considering implementing a victim call-back programme, it is recommended that police leaders carefully consider what further research may be required prior to deciding to implement such a programme. Costs need to be carefully weighed against potential benefits and the results of the trial show that that a carefully targeted programme is far more likely to be beneficial than a broad-brush approach. In order to accomplish this a multi-faceted and long-term strategy to implement such a programme considering both crime types and victim expectations will be required in order to elicit best results (Fixsen et al., 2005).

Conclusion

This trial shows that the intervention clearly had an impact on satisfaction levels for victims of crime. However, it also showed that the effects varied markedly depending on the type of the crime reported. It appears probable that differing victim expectations and characteristics explain this variance. The trial demonstrates that, if effectively targeted, interventions can potentially yield significant benefits. There are promising implications for police leaders and agencies who are considering victim satisfaction strategies – particularly in respect of BAME victims and young people, but challenges lie in understanding where and to whom those interventions are best applied. Further research is therefore strongly recommended in respect of not only understanding how differing crime types and
victim expectations may impact on interventions, but also around how victim characteristics play a part both in these expectations and in how an intervention can be targeted. If this is better understood, then police agencies can develop effective strategies that are likely to yield immediate and significant benefits at relatively low cost.

Bibliography

- Aihio, N. (2017). *Improving victim satisfaction in volume crime investigations: The role of police actions and victim characteristics.* PhD, London South Bank University.
- Ariel, B. (2019). *Statistical Power*. [Lecture to MSt in Applied Criminology and Police Management]. University of Cambridge. 18th July.
- Ariel, B. & Farrington, D. P. (2014). Randomized Block Designs. *In:* A. R. Piquero & D. Weisburd (eds.) *Handbook of Quantitative Criminology.* New York, NY: Springer.
- Ariel, B., Sutherland, A. & Sherman, L. W. (2019). 'Preventing treatment spillover contamination in criminological field experiments: the case of body-worn police cameras'. *Journal of Experimental Criminology*, 15:pp569-591.
- Avery, E. E., Hermsen, J. M. & Towne, K. (2020). 'Crime victimization distress, neighbourhood social cohesion and perceived police effectiveness'. *Journal of Community & Applied Social Psychology*, 10.1002/casp.2462.
- Barrett, G. A., Fletcher, S. M. G. & Patel, T. G. (2014). 'Black minority ethnic communities and levels of satisfaction with policing: Findings from a study in the north of England'. *Criminology & Criminal Justice*, 14:pp196-215.
- BBC (2019). Crimes solved by police in England and Wales at new low [Online]. London, UK: British Broadcasting Corporation. Available: <u>https://www.bbc.co.uk/news/uk-</u> <u>49029545#:~:text=The%20proportion%20of%20crimes%20solved,to%20be%20compiled%20</u> in%202015].
- BBC (2020) Covid-19: PM announces four-week England lockdown [Online]. London, UK: British Broadcasting Corporation. Available: https://www.bbc.co.uk/news/uk-54763956 [Accessed, https://www.bbc.co.uk/news/uk-54763956].
- Becker, L. A. (2000). *Effect Size Calculators* [Online]. Colorado Springs, Co: University of Colorado. Available: https://lbecker.uccs.edu/ [Accessed, https://lbecker.uccs.edu/].
- Bland, M. (2020). *Surveys and Questionnaires*. [Lecture to MSt in Applied Criminology and Police Management]. University of Cambridge.
- Bottoms, A. & Tankebe, J. (2012). 'Beyond Procedural Justice: A Dialogic Approach to legitimacy in Criminal Justice'. *The Journal of Criminal Law and Criminology*, 102:pp119-170.
- Bradford, B. (2010). *The Quality of Police Contact: Procedural Justice Concerns Among Victims of Crime in London*. [Jill Dando Institute of Security and Crime Science]. University College London.
- Bradford, B. (2011). 'Voice, neutrality and respect: Use of Victim Support services, procedural fairness and confidence in the criminal justice system'. *Criminology & Criminal Justice*, 11:pp345-366.

- Bradford, B. & Creutzfeld, N. (2018). 'Procedural justice in Alternative Dispute Resolution: Fairness judgments among users of Financial Ombudsman services in Germany and the United Kingdom'. *Journal of European Consumer and Market Law,* 7:pp188-200.
- Bradford, B., Jackson, J., Hough, M. & Farrall, S. (2008). Trust and Confidence in Criminal Justice: A Review of the British Research Literature. *In:* A. Jokinen, E. Ruuskanen, M. Yordanova, D. Markov & M. Ilcheva (eds.) *Review of Need: Indicators of Public Confidence in Criminal Justice.*
- Bradford, B., Jackson, J. & Stanko, E. A. (2009). 'Contact and confidence: revisiting the impact of public encounters with the police'. *Policing & Society*, 19:pp20-46.
- Brandl, S. G. & Horvath, F. (1991). 'Crime-victim evaluation of police investigative performance'. *Journal of Criminal Justice*, 19:pp109-121.
- Brown, R. & Smith, R. G. (2018). 'Exploring the relationship between organised crime and volume crime'. *Trends and Issues in Crime and Criminal Justice*, <<u>Go to</u> <u>ISI>://WOS:000452642900001</u>:1-15.
- Bryan-Brown, K. & Savill, T. (1997). *Cycle Theft in Great Britain*. [London, UK]. Transport Research Laboratory, Department for Transport.
- Cao, L. Q. (2015). 'Differentiating confidence in the police, trust in the police, and satisfaction with the police'. *Policing-an International Journal of Police Strategies & Management,* 38:pp239-249.
- Chandek, M. S. & Porter, C. O. L. H. (1998). 'The Efficacy of Expectancy Disconfirmation in Explaining Crime Victim Satisfaction With the Police'. *Police Quarterly*, 1:pp21-40.
- Chenerey, S., Holt, J. & Pease, K. (1997). *Biting Back: Preventing Repeat Burglary and Car Crime in Huddersfield.* Crime Prevention Unit Papers, UK Home Office.
- Circo, G., Melde, C. & McGarrell, E. F. (2019). 'Fear, victimization, and community characteristics on citizen satisfaction with the police'. *Policing-an International Journal of Police Strategies & Management*, 42:pp179-194.
- Cohen, J. (1988). *Statistical power analysis for the behavioral sciences,* [Hillsdale, N.J.] : Lawrence Erlbaum Associates, c1988.
- College of Policing. (2020). *Definition of Neighbourhoods Policing* [Online]. London, United Kingdom: College of Policing. Available: <u>https://www.college.police.uk/What-we-</u><u>do/Standards/Guidelines/Neighbourhood-Policing/Pages/definition-of-neighbourhood-policing.aspx</u>].
- Cope, N. (2004). "Intelligence led policing or policing led intelligence?' Integrating volume crime analysis into policing'. *British Journal of Criminology*, 44:pp188-203.
- Coupe, R. T. (2016). 'Evaluating the effects of resources and solvability on burglary detection'. *Policing & Society*, 26:pp563-587.

- Cross, C. (2016). ''I'm anonymous, I'ma voice at the end of the phone': A Canadian case study into the benefits of providing telephone support to fraud victims'. *Crime Prevention & Community Safety*, 18:pp228-243.
- Dai, M. Y. & Johnson, R. R. (2009). 'Is neighborhood context a confounder? Exploring the effects of citizen race and neighborhood context on satisfaction with the police'. *Policing-an International Journal of Police Strategies & Management,* 32:pp595-612.
- Daniels, S. A., Kelly, A., Bachand, D., Simeoni, E., Hall, C., Hofer, S. M. & Hayashi, A. (2016). 'Call to care: the impact of 24-hour postdischarge telephone follow-up in the treatment of surgical day care patients'. *American Journal of Surgery*, 211:pp963-966.
- Davenport, J. (2012). Car crime given higher priority than rape cases. *Evening Standard*, 13th April 2012, London UK.
- Desmond, M., Papachristos, A. V. & Kirk, D. S. (2016). 'Police Violence and Citizen Crime Reporting in the Black Community'. *American Sociological Review*, 81:pp857-876.
- Dudfield, G., Angel, C., Sherman, L. W. & Torrence, S. (2017). 'The Power Curve of Victim Harm: Targeting the Distribution of Crime Harm Index Values Across All Victims and Repeat Victims over 1 Year'. *Cambridge Journal of Evidence Based Policing*, 1:pp38-58.
- Dukes, R. L., Portillos, E. & Miles, M. (2009). 'Models of satisfaction with police service'. *Policing-an International Journal of Police Strategies & Management*, 32:pp297-318.
- Elliott, I., Thomas, S. D. M. & Ogloff, J. R. P. (2012). 'Procedural justice in contacts with the police: the perspective of victims of crime'. *Police Practice and Research*, 13:pp437-449.
- Enzmann, D. (2015). Notes on Effect Size Measures for the Difference of Means From Two Independent Groups: The Case of Cohen's d and Hedges' g. [Hamburg]. Institute of Criminal Sciences, University of Hamburg.
- Farrell, G. & Pease, K. (1993). Once Broken, Twice Bitten: Repeat victimisation and it's implications for crime prevention. Crime Prevention Unit Series Paper 46, Home Office Research Group, UK Home Office.
- Fixsen, D. L., Naoom, S. F., Blase, K. A., Friedman, R. M. & Wallace, F. (2005). Implementation Research: A Synthesis of the Literature. In: U. o. S. Florida (ed.) [Tampa, FL]. The National Implementation Research Network.

Foley, T. & Terrill, W. (2008). 'Police Comfort and Victims'. Victims & Offenders, 3:pp192-216.

- Forrester, D., Chatterton, M. & Pease, K. (1988). *The Kirkholt Burglary Prevention Project, Rochdale.* Crime Prevention Unit Papers, UK Home Office.
- Georgiev, G. Z. (2020). *Confidence Interval Calculator* [Online]. Available: <u>https://www.gigacalculator.com/calculators/confidence-interval-calculator.php</u>].
- Goudriaan, H., Wittebrood, K. & Nieuwbeerta, P. (2006). 'Neighbourhood characteristics and reporting crime Effects of social cohesion, confidence in police effectiveness and socio-economic disadvantage'. *British Journal of Criminology*, 46:pp719-742.

- Greater London Authority (2020). *London Borough Profiles & Atlas* [Online]. Islington, London: London Borough of Islington. Available: https://data.london.gov.uk/dataset/londonborough-profiles [Accessed 2020].
- Hellon, N (2021). Amateur bike detective puts brakes on Cambridge thieves. *The Sunday Times*, 31st January 2021.
- HMIC. (2014). *Policing in Austerity: Meeting the Challenge*. Her Majesty's Inspectorate of Constabulary, London, UK. <u>www.hmic.gov.uk</u>
- Hinds, L. & Murphy, K. (2007a). 'Public Satisfaction With Police: Using Procedural Justice to Improve Police Legitimacy'. *Australian & New Zealand Journal of Criminology*, 40:pp27-42.
- Hinkle, J. C., Weisburd, D., Famega, C. & Ready, J. (2013). 'The Problem Is Not Just Sample Size'. *Evaluation Review*, 37:pp213-238.
- Holdoway, S. (2010). 'Understanding 'Trust' and 'Confidence': Problems Within and Out with Constabularies'. *Policing*, 4:pp258-264.
- Hu, X., Dai, M., DeValve, M. J. & Lejeune, A. (2020). 'Understanding Public Attitudes towards the Police: Co-variates of Satisfaction, Trust, and Confidence'. *Canadian Journal of Criminology and Criminal Justice*, 62:pp26-49.
- Jackson, J., Bradford, B., Stanko, E. A. & Hohl, K. 2012. Reporting Crime. Just Authority? Trust in the Police in England & Wales. Oxford, UK: Routledge.
- Jackson, J. & Sunshine, J. (2007). 'Public Confidence in Policing: A Neo-Durkheimian Perspective'. *British Journal of Criminology*, 47:pp214-233.
- Jansson, K. (2005). Volume Crime Investigations: A review of the research literature. UK Home Office.
- Jonathan-Zamir, T., Mastrofski, S. D. & Moyal, S. (2015). 'Measuring Procedural Justice in Police-Citizen Encounters'. *Justice Quarterly*, 32:pp845-871.
- Joshi, A., Kale, S., Chandel, S. & Pal, D. K. (2015). 'Likert Scale: Explored and Explained'. *British Journal* of Applied Science & Technology, 7.
- Kule, A., Bumphus, V. W. & Iles, G. (2019). 'Intersectionality of race, class, and gender in predicting police satisfaction'. *Journal of Ethnicity in Criminal Justice*, 17:pp321-338.
- Kumar, T. V. (2018). 'Mediating influence of crime type on victim satisfaction with police services'. International Review of Victimology, 24:pp99-121.
- Laxminarayan, M., Bosmans, M., Porter, R. & Sosa, L. (2013). 'Victim Satisfaction with Criminal Justice: A Systematic Review'. *Victims & Offenders*, 8:pp119-147.
- Lee, H. D., Cao, L. Q., Kim, D. & Woo, Y. (2019). 'Police contact and confidence in the police in a medium-sized city'. *International Journal of Law Crime and Justice*, 56:pp70-78.

- London Borough of Camden (2020). *Camden Profile*. Strategy & Change unit, London Borough of Camden.
- Luhmann, N. (1988). Familiarity, Confidence, Trust. *In:* D. Gambetta (ed.) *Trust: Making and breaking cooperative relations.* Blackwells: Oxford, UK.
- MacQueen, S. & Bradford, B. (2016). 'Where did it all go wrong? Implementation failure—and more—in a field experiment of procedural justice policing'. *Journal of Experimental Criminology*, 13:pp321-345.
- Maddox, L., Lee, D. & Barker, C. (2011). 'Police Empathy and Victim PTSD as Potential Factors in Rape Case Attrition'. *Journal of Police Criminal Psychology*, 26:pp112-117.
- Mathew, A. S., Agarwal, J. P., Munshi, A., Laskar, S. G., Pramesh, C. S., Karimundackal, G., Jiwnani, S., Prabhash, K., Noronha, V., Joshi, A., Rangarajan, V., Purandare, N. C., Jambhekar, N., Tandon, S., Mahajan, A., Kumar, R. & Deodhar, J. (2017). 'A prospective study of telephonic contact and subsequent physical follow-up of radically treated lung cancer patients'. *Indian Journal of Cancer*, 54:pp241-252.
- Mazerolle, L. G., Bennett, S., Davis, J., Sargeant, E. & Manning, M. (2013). 'Procedural justice and police legitimacy: a systematic review of the research evidence'. *Journal of Experimental Criminology*, 9:pp245-274.
- Ministry of Justice (2015). *Code of Practice for Victims of Crime. In:* Ministry of Justice (ed.) HMSO, London, UK.
- MOPAC. (2020a). Local Volume Crime Priorities Dashboard. Mayor's Office for Policing and Crime. <u>https://www.london.gov.uk/what-we-do/mayors-office-policing-and-crime-mopac/data-and-statistics/crime-dashboard</u>
- MOPAC. (2020b). *Public Voice Dashboard*. Mayor's Office for Policing and Crime. <u>https://www.london.gov.uk/what-we-do/mayors-office-policing-and-crime-mopac/data-and-statistics/public-voice-dashboard</u>
- MPS (2017). *Metropolitan Police Crime Assessment Policy*. [London, United Kingdom]. FOIA Unit, Metropolitan Police Service.
- MPS (2020a). Crime Data Dashboard [Online]. London, UK: Data & Insight Unit, Metropolitan Police Service. Available: https://www.met.police.uk/sd/stats-and-data/met/crime-datadashboard/ [Accessed, <u>https://www.met.police.uk/sd/stats-and-data/met/crime-datadashboard/</u>].
- MPS (2020b). *Police Officer Pay Costs from September 2020*. [London, United Kingdom]. Flnance Services, Metropolitan Police Service.
- MPS (2020c). User Satisfaction Survey: Overview of results and changes in Survey Methodology. [London, United Kingdom]. Reporting & Analytics Data Office, Metropolitan Police Service.
- MPS (2020d). What happens after you report a crime? [Online]. London, UK: Metropolitan Police Service. Available: https://www.met.police.uk/advice/advice-and-information/acr/after-you-

report-a-crime/ [Accessed, <u>https://www.met.police.uk/advice/advice-and-information/acr/after-you-report-a-crime/]</u>.

- Myhill, A. & Bradford, B. (2012). 'Can police enhance public confidence by improving quality of service? Results from two surveys in England and Wales'. *Policing and Society*, 22:pp397-425.
- Nestor, E. (2020) *The Met Police and issues surrounding racism,* 2020. BBC Radio London Directed by Nestor, E.: British Broadcasting Corporation. <u>https://www.bbc.co.uk/programmes/p08kcvbs/</u>
- Neyroud, P. (2019). *Learning from and Leading Testing in Policing*. [Lecture to MSt in Applied Criminology and Police Management]. University of Cambridge. 2 April.
- Neyroud, P. W (2017). *Learning to Field Test in Policing: Using an analysis of completed randomised control trials involving the police to develop a theory on the factors contributing to high levels of treatment integrity in police field experiments.* PhD, University of Cambridge.
- Norris, F. H. & Thompson, M. P. (1993). 'The Victim in the System: The Influence of Police Responsiveness on Victim Alienation'. *Journal of Traumatic Stress*, 6:pp515-532.
- ONS (2017). Women and the Criminal Justice System [Online]. Office for National Statistics, London, UK. Available: <u>https://www.gov.uk/government/statistics/women-and-the-criminal-justice-system-2017</u>
- ONS (2018a). Crime in England and Wales: Year ending December 2018. [Online] Office for National Statistics, London, UK. Available :<u>https://www.ons.gov.uk/peoplepopulationandcommunity/crimeandjustice/bulleti</u>ns/crimeinenglandandwales/yearendingdecember2018#overview-of-crime
- ONS (2018b). *UK Population by Ethnicity* [Online]. Office for National Statistics, London, UK. Available: <u>https://www.ethnicity-facts-figures.service.gov.uk/uk-population-by-ethnicity</u>
- ONS (2020). Population Profiles for Local Authorities in England [Online]. Office for National Statistics, London, UK. Available: <u>https://www.ons.gov.uk/peoplepopulationandcommunity/populationandmigration/populationestimates/articles/populationprofilesforlocalauthoritiesinengland/2020-12-14</u>
- Pease, K. & Farrell, G (2016). Repeat Victimisation. *In:* R.Wortley & M. Townsley (eds.) *Environmental Criminology and Crime Analysis.* Collumpton: Willan.
- Reisig, M. D. & Chandek, M. S. (2001). 'The effects of expectancy disconfirmation on outcome satisfaction in police-citizen encounters'. *Policing: An International Journal of Police Strategies & Management*, 24:pp88-99.
- Rosenbaum, D., Lawrence, D. S., Hartnett, S. M., McDevitt, J. & Posick, C. (2015). 'Measuring procedural justice and legitimacy at the local level: the police-community interaction survey'. *Journal of Experimental Criminology*, 11:pp335-366.
- Ruane, J. M. (2005). Some Perfectly Valid Points: Measurement, Internal and External Validity. *Essentials of Research Methods: A Guide to Social Science Research*. Maldon, Ma: Blackwell Publishing.

- Schulz, K. F., Altman, D. G. & Moher, D. (2010). 'CONSORT 2010 Statement: updated guidelines for reporting parallel group randomised trials'. *Trials*, 11:32.
- Shapland, J. & Hall, M. (2007). 'What do we know about the effects of crime on victims?'. International Review of Victimology, 14:pp175-217.
- Sherman, L. W. (2017). *Targeting, Testing and Tracking: The CAM System of Evidence-Based Police Assignment*. [The Cambridge Centre for Evidence Based Policing Ltd].
- Sherman, L. W. (2019). *Testing Police Practices: The (Maryland) Scientific Methods Scale*. [Lecture to MSt in Applied Criminology and Police Management]. University of Cambridge. 9 April.
- Sherman, L. W., Gottfredson, D. C., MacKenzie, D. L., Eck, J., Reuter, P. & Bushway, S. D. (1998). *Preventing Crime: What Works, What Doesn't, What's Promising* US Department of Justice, Washington DC.
- Sherman, L. W. & Strang, H. (2019). *Introduction to Triple-T for Victims*. [Lecture to MSt in Applied Criminology and Police Management]. University of Cambridge. 8th July 2019.
- Sherman, L. W. & Weisburd, D. (1995). 'General deterrent effects of police patrol in crime "hot spots": A randomized, controlled trial'. *Justice Quarterly*, 12:pp625-648.
- Skogan, W. G. (2006). 'Asymmetry in the Impact of Encounters with Police'. *Policing & Society*, 16:pp 99-126.
- Skogan, W. G. & Wycoff, M. A. (1987). 'Some Unexpected Effects of a Police Service for Victims'. *Crime & Delinquency*, 33:pp490-501.
- Slothower, M., Sherman, L. W. & Neyroud, P. (2015). 'Tracking Quality of Police Actions in a Victim Contact Program: A Case Study of Training, Tracking, and Feedback (TTF) in Evidence-Based Policing'. International Criminal Justice Review, 25:pp98-116.
- Stanko, E. A. & Bradford, B. (2009). 'Beyond Measuring 'How Good a Job' Police Are Doing: The MPS Model of Confidence in Policing'. *Policing*, 3:pp322-330.
- Stripe, N. (2020). *Coronavirus and crime in England and Wales: August 2020. Crime and Criminal Justice Bulletin.* Office for National Statistics, London, UK. 26th August 2020.
- Sunshine, J. & Tyler, T. R. (2003). 'The Role of Procedural Justice and Legitimacy in Shaping Public Support for Policing'. *Law & Society Review*, 37:pp513-548.
- SurveyMonkey (2020). Sample Size Calculator [Online]. United Kingdom: SurveyMonkey. Available: https://www.surveymonkey.co.uk/mp/sample-size-calculator/
- Tankebe, J (2014). Police Legitimacy. *The Oxford Handbook of Police and Policing*. Oxford University Press.
- Tuffin, R., Morris, J. & Poole, A. (2006). An evaluation of the impact of the National Reassurance Policing Programme. Home Office Research, Development and Statistics Directorate, London UK.

- Turner, A. G (1981). The San Jose Recall Study. *In:* R. G. Lehnen & W. G. Skogan (eds.) *The National Crime Survey Working Papers Volume 1: Current and Historical Perspectives.* US Department of Justice, Washington DC.
- Tyler, T. R. (1988). 'What is Procedural Justice? Criteria used by Citizens to assess the fairness of legal procedures'. *Law and Society Review*, 22:pp103-135.
- Tyler, T. R. (2003). 'Procedural Justice, Legitimacy, and the Effective Rule of Law'. *Crime and Justice,* 30:pp283-357.
- Tyler, T. R. (2004a). 'Enhancing Police Legitimacy'. *The Annals of the American Academy of Political Science*, 593:pp84-99.
- Tyler, T. R. (2004b). 'Enhancing Police Legitimacy'. *The ANNALS of the American Academy of Political and Social Science*, 593:pp84-99.
- van Dijk, J. J (2001). Attitudes of Victims and Repeat Victims toward the Police: Results of the International Crime Victims Survey. *Repeat Victimization*. Crime Prevention Studies, Criminal Justice Press, Monsey, New Jersey.
- Walijee, H., Sood, S., Markey, A., Krishnan, M., Lee, A. & De, S. (2020). 'Is nurse-led telephone followup for post-operative obstructive sleep apnoea patients effective? A prospective observational study at a paediatric tertiary centre'. *International Journal of Pediatric Otorhinolaryngology*, 129.

Appendix A: Briefing for officers conducting interventions

Central North BCU victim call-back trial Information for officers/staff engaged in victim call backs

Introduction

Firstly, thank you for your assistance in taking part in this trial. The results will inform MPS and MOPAC policy about contacting victims across the MPS and into wider policing nationally, so it is vital that the calls to victims are done by local officers who can provide that 'personal touch.' That is where you come in...

The background

Over a ten-week period, about 50% of CN's victims of screened out TFMV and Cycle Theft will be randomly selected to get a reassurance call. This equates to about 700 victims or about 10 calls a day.

Officers from CN Ops or LRT (as decided by SLT) will conduct the calls. The crimes will have been reported around 1-2 weeks previously. This is to allow enough time for a screening decision to be made and a victim of crime letter to be sent.

Screened in crimes or those where the victim has not provided contact details will be excluded, and the victims selected to get a call will be automatically randomly chosen.

A spreadsheet of those victims who are set to receive a call will then be provided on a weekly basis.

The idea of the call is to provide a personal contact to the victim, to demonstrate that someone has looked at their report, to provide crime prevention advice and reassurance as appropriate.

A couple of weeks later, an independent member of staff will telephone victims and ask survey questions to assess the levels of satisfaction. They will also be telephoning those victims who did not

get a call and asking the same questions. Any differences in satisfaction/confidence can then be analysed in the knowledge that the only differing factor between the two sets of victims was the telephone call made by CN officers – hence why your work is so important.

How to conduct the call backs – a step by step guide.

Ensure you have access to the spreadsheet on box and can edit it.

Open and look at the CRIS report – make sure that you know any specific details about the incident so that you come across as having personal knowledge of the case.

Telephone the victim – their details are on the spreadsheet – A conscious decision has been made not to have a 'script' – there is a risk that you will come across as sounding 'wooden' and this may be counterproductive. Instead, here are some pointers for the call:

a) Introduce yourself. Ensure that you tell the victim you work in the local police station/command where the offence took place.

b) Explain you have read their crime report and that you want to provide some follow up contact.

c) Sympathise and empathise with their situation – e.g., saying you are sorry for the loss/inconvenience

d) Explain that although the crime report has been screened out, the report will be used by local officers on patrol and will feed into intelligence and patrol tasking processes.

e) Explain that the local DWOs and NPT will also be made aware and knowledge of local crimes will help their work and engagement activities.

f) Offer crime prevention advice – Plug immobilise.com or advise around property marking etc.

g) Offer any other help that may be appropriate - e.g., victim support contact details

h) Explain that they will receive a victim of crime letter with further details if not already received.

i) Provide contact details for the CMU if they have further questions – 020 7230 3400 (0800-2100 hours) – they will need their CRIS number.

Finally, if not on the spreadsheet, *please make an effort to obtain the victim's self-defined ethnicity* – this is because a bug on the TDIU process means that this question doesn't get asked at time of reporting.

Once the call has finished, please note the spreadsheet with your shoulder number and the fact the call has been completed together with any notes/comments in the relevant fields, including the self-defined ethnicity details as above.

FAQs...

What do I do if the victim raises an issue or complaint?

Take details and explain you will refer to the OIC. Create a CRIS memo and email the OIC. If there is an immediate threat/harm or risk identified, keep the caller on the line and seek advice from a supervisor.

What if the victim doesn't answer?

Note the spreadsheet with the attempt but try again later. Each victim will need at least 3 attempts at differing times of day. Consider prefixing your call with 1848 if the phone doesn't accept withheld numbers.

When should I be calling the victims?

Call between 0800 and 2100 hours. Do not call outside of these times.

What if the victim gives me intelligence about other crimes?

Take details and create a CRIMINT report as appropriate.

I have an idea to make the trial better?

Every ten days the trial will be reviewed in a meeting with CN officers. Email CI Dearden with your suggestion and it will be added to the next review.

Should I tell the victims they will get a follow-up survey?

No – this may sway their views at the survey and skew the results.

What if the victim wants to praise or thank me?

Note the details on the spreadsheet – their comments will be included in the final report if appropriate.

Conclusion

The trial will take place over ten weeks with the surveys finishing about two weeks after the call backs. The results will be analysed and will be published in the New Year. A summary will be given to CN officers involved in the trial and the results will be presented to victim's board (chaired at DAC level) and to MOPAC to inform MPS policy going forward.

Once again, thank you for your support in this part of the trial.

Ben Clark

Superintendent 200725

Appendix B: Survey Script

Central North Victim Satisfaction Trial

Script for Satisfaction Survey – Ver1_1

(S1) Instructions to officer/staff conducting telephone surveys.

The survey is designed to capture the views of victims of Cycle theft and Thefts from motor vehicles in Camden and Islington. The vast majority of these crimes are screened out and this survey is concerned with trying to identify ways to improve confidence and satisfaction among these victims.

Before making a survey call to a victim, please ensure that you have the survey spreadsheet open in front of you and you are able to scroll across into the relevant columns. The vast majority of columns are drop down menus, but there are some free text entries.

You will be reading from this script, so it is advisable to either have it open on another screen or to print the script off. You are strongly advised to read this script through before making calls to victims.

The script will instruct you to go through in particular ways depending on the responses – these are marked in blue and will either be shown as S(Sections) or Q(Questions).

Please note the FAQs page at the end, which contains information that victims sometimes request.

If you have to arrange a call-back because the victim cannot speak at that particular time, it is your responsibility to make the call back or arrange for someone to do so – please do not assume someone else will deal.

If the number you are calling does not accept withheld numbers, please dial 1848 before the number (e.g. 020 8721 2300 becomes 1848 020 8721 2300) – you do not need to add a '9' for an outside line in this instance.

When you are ready to call a victim please go to S2S2

Attempt call -

Say: "Good (PeriodOfDay), could I speak to (Victim first name) (Victim last name) please?

(If not immediately available, try to make call back arrangements - see S2)

My name's (Operator:firstName) (Operator:lastName). I'm calling from Central North BCU in the Metropolitan Police about the crime you reported on (CRIS:REPORTED DATE).

I would like to ask a few questions to get your feedback about how satisfied you were with the service you received.

(If respondent does not remember incident –It was about a/an (CRIS:Current Classification))

The feedback you provide will be used by both the local Management team and the Metropolitan Police to improve the service given by the police to future victims of crime.

I will only ask about your contact with the police and the questionnaire takes around 5 minutes."

Response:

Yes – Continue – Go to S2

No – No time now – Go to S1a

No - Refused - Go to S1b

Insufficient English/Interpreter Required – Go to S1b

No reply repeated ringing – Go to S1c

Answerphone – Go to S1c

Engaged – Go to S1c

Unobtainable – Go to S1d

Fax machine/Modem – Go to S1d

Wrong number/never heard of victim – Go to S1b

Can't remember/never contacted police – Go to S1b

Other (specify) – Go to S1b

- **S1a** Arrange a time and date for you to make a call back note Column J in spreadsheet accordingly.
- S1b Thank the respondent for their time and close. Note Column J in spreadsheet with drop down reason the call could not be completed.
- S1c Note attempt in column J. Leave voicemail message if able explaining call and that you will call back. Each respondent should have 3 x attempts at differing times before closure.
 Consider unmasking withheld numbers on 3rd attempt. If still no response after 3rd attempt, close and note Column J in spreadsheet with drop down reason.
- S1d Check number and retry if necessary. If still unobtainable or fax/modem, close and noteColumn J with drop down reason.

S2

If at any point the victim wants to finish the call before the survey is complete, please go to S4.

S2a Say 'Before we begin, I need to ask, are you currently driving?'

Clarify as necessary

Yes – Go to S1a above

No – in a car but not driving – Go to S2b

No - not in a car - Go to S2b

S2b

Say ' As I mentioned in the introduction, I am (Say name) calling from Camden and Islington police and I would like to ask you some questions in respect of how satisfied you were in respect of the crime that you reported. Your answers will be kept for statistical purposes, but they will be anonymised. Any personal data will only be kept by the MPS in line with GDPR in respect of the crime that you reported. If you want more information, please go to

https://www.met.police.uk/privacy-notice/

If you would like to confirm my identity, I can give my MPS email address through which you can contact me, or you can call the Metropolitan Police contact centre on 101 and they can confirm who I am."

Go to S2c

S2c

Ask "Are you 16 years old or over?"

Yes – Go to Q3

No – Say 'Thank you for your time but we are unable to conduct surveys with people in your age group.' **Go to S1b above**.

Q1

ASK: "Thinking about the crime you reported (Clarify details if needed), were you satisfied,

dissatisfied or neither with the actions taken by the police at the time you reported it?

And is that completely, very or fairly (if not 'neither')"

Code Column K(Q1):

Completely satisfied	7
Very satisfied	6
Fairly satisfied	5
Neither satisfied/Nor dissatisfied	4
Fairly dissatisfied	3
Very dissatisfied	2
Completely dissatisfied	1
Don't know	98
Refused to answer	99

Go to Q2

Q2

ASK: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with any follow up actions from the police?

And is that completely, very or fairly (if not 'neither')"

Code Column L(Q2):

Completely satisfied 7 Very satisfied 6 Fairly satisfied 5 Neither satisfied/Nor dissatisfied 4 Fairly dissatisfied 3 Very dissatisfied 2 Completely dissatisfied 1 Don't know 98 Refused to answer 99

Go to Q3

Q3

ASK: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with how you were kept informed by the police?

And is that completely, very or fairly (if not 'neither')"

Code Column M(Q3):

Completely satisfied	7
Very satisfied	6
Fairly satisfied	5
Neither satisfied/Nor dissatisfied	4
Fairly dissatisfied	3
Very dissatisfied	2
Completely dissatisfied	1
Don't know	98
Refused to answer	99

Go to Q4

Q4

ASK: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with how

you were updated by the police?

And is that completely, very or fairly (if not 'neither')"

Code Column N(Q4):

Completely satisfied 7

Very satisfied 6

Fairly satisfied	5
Neither satisfied/Nor dissatisfied	4
Fairly dissatisfied	3
Very dissatisfied	2
Completely dissatisfied	1
Don't know	98
Refused to answer	99

Go to Q5

Q5

ASK: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with how

you were treated by the police?

And is that completely, very or fairly (if not 'neither')"

Code Column O(Q5):

Completely satisfied	7
Very satisfied	6
Fairly satisfied	5
Neither satisfied/Nor dissatisfied	4
Fairly dissatisfied	3
Very dissatisfied	2

Completely dissatisfied	1
Don't know	98
Refused to answer	99

Go to Q6

Q6

ASK: "Thinking about the crime you reported, were you satisfied, dissatisfied or neither with the

overall service you received?

And is that completely, very or fairly (if not 'neither')"

Code Column P(Q6):

Completely satisfied	7
Very satisfied	6
Fairly satisfied	5
Neither satisfied/Nor dissatisfied	4
Fairly dissatisfied	3

- Very dissatisfied 2
- Completely dissatisfied 1
- 99
- Refused to answer

Go to Q7

Don't know

98

Q7

"Did the service you received from the police exceed, meet or fall below your expectations?"

Code Column Q(Q7):

Exceeded expectations3Met expectations2Fell below expectations1Don't know98Refused99

Go to Q8

Q8

"Prior to this incident was your general opinion of the police high, low or mixed?"

Code Column R(Q8):	
Generally High	3
Mixed	2
Generally Low	1
Don't know	98
Refused	99

Go to Q9

Q9

"As a result of this incident, has your opinion of the police got better, got worse or not changed?"

Code Column S(Q9):

Got better	3
Not changed	2
Got worse	1
Don't know	98
Refused	99

Go to Q10

Q10

Say "Under the Equalities Act 2010, the Metropolitan Police Service as an organisation has a duty to prevent discrimination and ensure a fair service to all. Therefore, we'd like to ask the following questions about you, which will also help us to improve our service to victims in Camden & Islington. Please note, all of these questions are entirely optional."

"Firstly, I'd just like to check that the details on the crime report are correct."

Confirm Age from crime report details – change details if necessary, in Column T(Q10).

Go to Q11

Q11

Confirm Gender from crime Report details. Change details if necessary, in Column U(Q11).

Go to Q12

Q12

Say "How would you define your ethnic background?"

Use SD ethnicity codes – clarify if necessary. Code Column V(Q12) from drop-down:

A1 INDIAN

A2 PAKISTANI

A3 BANGLADESHI

A9 ANY OTHER ASIAN BACKGROUND

BLACK OR BLACK BRITISH

B1 CARIBBEAN

B2 AFRICAN

B9 ANY OTHER BLACK BACKGROUND

MIXED

M1 WHITE & BLACK CARIBBEAN

M2 WHITE & BLACK AFRICAN

M3 WHITE & ASIAN

M9 ANY OTHER MIXED BACKGROUND

CHINESE OR OTHER ETHNIC GROUP

O1 CHINESE

O9 ANY OTHER ETHNIC GROUP

WHITE

W1 BRITISH

W2 IRISH

W9 ANY OTHER WHITE BACKGROUND

N3 The person did not understand what is required

N4 The person declined to define their ethnicity

Go to Q13

Q13

Please can you tell me your occupation?

Write Free Text in Column W(Q13).

Go to Q14

Q14

Say "The Equality Act 2010 defines a person as having a disability if he or she 'has a physical or mental impairment, which has a substantial and long-term adverse effect on his or her ability to carry out normal day to day activities'. Do you have such a disability?"

Code Column X(Q14) from drop down:

Yes

No

Don't know

Declined to answer

Go to Q15

Q15

This concludes the victim satisfaction survey. I am sorry that you were a victim of crime in Camden & Islington. Is there anything you would like to say or add in respect of the service that you received from the police?

Yes	Fill in free text box in Column Y(Q15) with comments.
No	Leave Column Y(Q15) blank.
Go to S4 below	

S4

Thank the participant for their time and close the call

If incomplete because the victim finished the call early, please note in Column J.

Update the spreadsheet and save back into Box.

FAQs if required:

How can respondents verify our identity: If you would like to confirm my identity, I can supply you with my metropolitan police service email address or the contact number for our main control room and they will be happy to confirm my identity. If respondent wishes, advise them to call 101 to verify identity.

I would like an update on my case: If a respondent wants an update on their case then please tell them to contact their case officer or call 101.

GDPR/Data issues: What we learn from this feedback helps us make sure the Metropolitan Police are providing the best service possible to you. Any information that you provide will be processed by the MPS in line with the requirements of the UK Data Protection Act and the EU General Data Protection Regulation (GDPR). For more information, please go to https://www.met.police.uk/privacy-notice/ or contact the MPS data office at mpsdataoffice@met.police.uk.

What is Q99 on the spreadsheet? This is the CRIS readout as to whether a victim was a repeat victim in the last 12 months. You do not need to ask the victim this question as part of the survey.

	Block A: Vehicle Crime	Block B: Cycle Theft
Q1: Time of reporting	0.203	0.220
Q2: Follow-up actions	0.394	0.076*
Q3: Kept informed	0.999	0.023**
Q4: Kept updated	0.945	0.001***
Q5: Treatment	0.182	0.001***
Q6: Overall service	0.288	0.006***
Q7: Met expectations	0.808	0.052*
Q8: Prior opinion	0.840	0.017***
Q9: Opinion now	0.399	0.058*

Appendix C: Statistical significance levels of questionnaire responses

* p < 0.10; **p < 0.05; *** p < .01

Appendix D: Sample sizes, mean responses & standard deviation results for sub-groups

		Block A Control (n)	Block A Treatment (n)	Block B Control (n)	Block B Treatment (n)
Condor	Male	87	79	63	82
Gender	Female	57	42	37	28
Self-defined	White	94	70	72	94
Ethnicity	BAME	39	43	17	28
Repeat victir	Repeat victim	24	13	13	16
victimisation	1 st time victim	105	103	78	95
	24 and under	9	8	13	18
A	25-34 years	37	30	46	58
Age	35-44 years	32	32	19	31
	45-54 years	25	26	15	10
	55 and over	42	23	8	7

TABLE 1: SAMPLE SIZES OF SUBGROUPS FOR ANALYSIS

		Block A Male		Block A Female		Block B Male		Block B Female	
		Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Q1: Time of	Mean	4.364	4.188	4.912	4.690	4.286	4.646	4.135	4.171
reporting	St Dev	1.400	1.681	1.720	1.225	1.506	1.501	1.436	1.247
Q2: Follow-	Mean	4.182	4.652	4.477	4.548	3.921	4.469	4.027	4.195
up actions	St Dev	1.474	1.760	1.686	1.383	1.515	1.618	1.284	1.173
Q3: Kept	Mean	4.239	4.826	4.789	4.619	4.254	4.805	4.243	4.357
informed	St Dev	1.552	1.761	1.724	1.290	1.553	1.443	1.194	1.250
Q4: Kept	Mean	4.261	4.870	4.719	4.643	4.238	4.988	4.189	4.452
updated	St Dev	1.570	1.777	1.735	1.411	1.455	1.357	1.411	1.418
Q5:	Mean	4.955	5.043	5.228	4.929	4.460	5.232	4.639	4.929
Treatment	St Dev	1.260	1.601	1.522	1.334	1.467	1.337	1.377	1.421
Q6: Overall	Mean	4.568	4.870	4.807	4.762	4.210	4.902	4.297	4.429
service	St Dev	1.452	1.701	1.762	1.394	1.525	1.519	1.486	1.312
Q7: Met	Mean	1.890	1.709	1.889	1.902	1.814	1.911	1.686	1.902
expectations	St Dev	0.796	0.599	0.598	0.576	0.700	0.508	0.574	0.576
Q8: Prior	Mean	2.386	2.440	2.544	2.488	2.393	2.494	2.472	2.561
opinion	St Dev	0.637	0.637	0.532	0.546	0.557	0.611	0.600	0.543
Q9: Opinion	Mean	2.023	1.987	2.053	2.049	1.952	2.098	1.973	2.098
now	St Dev	0.371	0.515	0.544	0.379	0.489	0.509	0.545	0.532

TABLE 2: MEAN RESPONSES AND STANDARD DEVIATIONS BY GENDER

Block A		24 and		25-34		35-44		45-54		55 and over	
Control/Treatment		С	Т	С	Т	C	Т	С	Т	С	Т
(C/T)											
Q1: Time of	Mean	4.778	3.750	4.405	4.200	4.656	4.156	4.360	3.962	4.714	5.174
reporting	St Dev	1.474	1.639	1.716	1.492	1.593	1.394	1.353	1.870	1.436	1.167
Q2: Follow	Mean	5.000	3.750	4.270	4.200	4.375	4.156	4.200	3.923	4.524	4.783
up actions	St Dev	1.054	1.479	1.655	1.301	1.691	1.523	1.600	1.880	1.435	1.214
Q3: Kept	Mean	5.000	3.875	4.324	4.467	4.500	4.344	4.040	4.077	4.667	5.043
informed	St Dev	1.155	1.615	1.801	1.147	1.601	1.428	1.685	1.796	1.425	1.233
Q4: Kept	Mean	4.444	3.875	4.486	4.586	4.688	4.313	4.120	4.000	4.429	5.043
updated	St Dev	1.423	1.615	1.780	1.160	1.550	1.424	1.751	1.922	1.417	0.999
Q5:	Mean	5.333	4.625	5.000	4.690	5.125	4.656	4.680	4.538	5.167	5.391
Treatment	St Dev	1.054	1.867	1.577	1.342	0.960	1.383	1.406	1.737	1.430	0.920
Q6: Overall	Mean	5.111	3.625	4.649	4.367	4.688	4.562	4.200	4.000	4.786	5.043
service	St Dev	1.370	1.409	1.712	1.378	1.446	1.391	1.414	1.922	1.612	1.301
Q7: Met	Mean	2.250	1.625	1.912	1.621	1.690	1.774	1.667	1.615	2.100	2.087
expectations	St Dev	0.433	0.484	0.562	0.552	0.532	0.551	0.471	0.625	0.995	0.583
Q8: Prior	Mean	2.500	2.000	2.200	2.379	2.400	2.586	2.480	2.360	2.643	2.609
opinion	St Dev	0.500	0.535	0.668	0.611	0.554	0.558	0.640	0.686	0.479	0.488
Q9: Opinion	Mean	2.000	2.000	2.027	2.000	1.938	1.969	2.000	1.840	2.143	2.261
now	St Dev	0.000	0.000	0.434	0.525	0.496	0.467	0.283	0.463	0.467	0.529

TABLE 3: MEAN RESPONSES AND STANDARD DEVIATIONS BY AGE GROUP - BLOCK A

Block B		24 and under		25-34		35-44		45-54		55 and over	
Control/Treatment (C/T)		С	т	С	Т	С	Т	С	Т	С	Т
Q1: Time of	Mean	4.077	4.944	4.348	4.466	3.368	4.323	4.267	4.200	5.375	5.143
reporting	St Dev	1.439	1.393	1.433	1.429	1.265	1.446	1.389	1.661	0.857	0.350
Q2: Follow	Mean	3.769	4.611	3.913	4.316	3.526	4.387	4.133	4.000	4.875	5.000
up actions	St Dev	1.625	1.297	1.299	1.602	1.464	1.430	1.408	1.483	0.781	0.926
Q3: Kept	Mean	3.846	5.000	4.217	4.576	4.053	4.742	4.267	4.200	5.000	5.000
informed	St Dev	1.610	1.106	1.382	1.586	1.356	1.046	1.436	1.661	0.707	0.926
Q4: Kept	Mean	3.846	5.444	4.217	4.610	3.947	5.000	4.133	4.300	5.125	5.000
updated	St Dev	1.561	1.066	1.366	1.551	1.394	1.016	1.454	1.735	0.781	0.926
Q5:	Mean	4.000	5.444	4.622	5.119	4.316	5.161	4.400	4.600	5.250	5.000
Treatment	St Dev	1.710	1.383	1.371	1.439	1.340	1.167	1.405	1.685	0.661	0.756
Q6: Overall	Mean	4.000	5.167	4.311	4.644	3.632	4.839	4.200	4.000	5.375	5.286
service	St Dev	1.617	1.462	1.443	1.459	1.346	1.393	1.424	1.732	0.992	0.700
Q7: Met	Mean	1.692	2.059	1.738	1.825	1.684	2.032	1.643	1.700	2.571	2.167
expectations	St Dev	0.606	0.539	0.440	0.463	0.729	0.538	0.610	0.640	1.050	0.373
Q8: Prior	Mean	2.091	2.588	2.372	2.517	2.722	2.581	2.462	2.300	2.500	2.429
opinion	St Dev	0.514	0.492	0.572	0.565	0.448	0.610	0.499	0.781	0.707	0.495
Q9: Opinion	Mean	1.923	2.222	1.933	2.000	1.789	2.161	2.067	2.100	2.375	2.286
now	St Dev	0.474	0.533	0.442	0.455	0.521	0.514	0.573	0.700	0.484	0.452

TABLE 4: MEAN RESPONSES AND STANDARD DEVIATIONS BY AGE GROUP - BLOCK B

		Bloo Rep vict	ck A Deat ims	Block / rep vict	A Non- eat ims	Bloo Rep vict	ck B Jeat ims	Block B Non- repeat victims	
		Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Q1: Time of	Mean	4.083	4.308	4.676	4.408	4.231	3.813	4.256	4.621
reporting	St Dev	1.956	1.813	1.444	1.523	1.527	1.775	1.506	1.370
Q2: Follow-	Mean	4.167	4.308	4.533	4.282	3.538	3.813	4.103	4.463
up actions	St Dev	1.972	1.682	1.435	1.535	1.599	1.775	1.410	1.328
Q3: Kept	Mean	4.250	4.692	4.533	4.740	3.615	3.875	4.308	4.740
informed	St Dev	1.920	1.435	1.550	1.293	1.778	1.932	1.362	1.256
Q4: Kept	Mean	4.208	4.769	4.533	4.466	3.462	4.188	4.359	4.865
updated	St Dev	1.936	1.367	1.550	1.487	1.906	2.038	1.330	1.276
Q5:	Mean	4.667	4.923	5.171	4.796	3.769	4.125	4.545	5.208
Treatment	St Dev	1.724	1.492	1.230	1.451	1.804	1.728	1.344	1.191
Q6: Overall	Mean	4.208	4.154	4.810	4.510	3.833	4.063	4.321	4.833
service	St Dev	1.848	1.915	1.435	1.494	1.675	1.676	1.498	1.327
Q7: Met	Mean	1.714	1.917	1.949	1.757	1.667	1.733	1.781	1.957
expectations	St Dev	0.547	0.493	0.761	0.599	0.624	0.573	0.687	0.500
Q8: Prior	Mean	2.304	2.462	2.465	2.469	2.273	2.312	2.431	2.574
opinion	St Dev	0.621	0.746	0.573	0.593	0.617	0.768	0.573	0.487
Q9: Opinion	Mean	2.042	1.833	2.057	2.029	1.917	1.875	1.962	2.126
now	St Dev	0.455	0.373	0.435	0.474	0.640	0.484	0.517	0.528

TABLE 5: MEAN RESPONSES AND STANDARD DEVIATIONS BY REPEAT VICTIMISATION STATUS
		Bloo Wł vict	ck A nite ims	Bloo BA vict	ck A ME ims	Bloo Wł vict	ck B nite ims	Bloo BA vict	ck B ME ims
		Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Q1: Time of	Mean	4.691	4.371	4.000	4.326	4.292	4.468	4.118	4.714
reporting	St Dev	1.459	1.426	1.617	1.851	1.476	1.389	1.604	1.555
Q2: Follow-	Mean	4.454	4.286	4.000	4.279	4.083	4.366	3.765	4.571
up actions	St Dev	1.492	1.416	1.585	1.783	1.402	1.480	1.592	1.498
Q3: Kept	Mean	4.585	4.529	3.923	4.442	4.333	4.621	4.000	4.964
informed	St Dev	1.505	1.370	1.670	1.647	1.344	1.393	1.680	1.322
Q4: Kept	Mean	4.532	4.429	4.077	4.628	4.264	4.737	4.235	5.036
updated	St Dev	1.569	1.430	1.655	1.556	1.404	1.401	1.516	1.349
Q5:	Mean	5.138	4.829	4.692	4.907	4.648	5.063	4.588	5.321
Treatment	St Dev	1.277	1.383	1.488	1.654	1.344	1.375	1.647	1.338
Q6: Overall	Mean	4.755	4.486	4.128	4.558	4.417	4.716	3.764	4.964
service	St Dev	1.528	1.442	1.588	1.702	1.507	1.412	1.554	1.636
Q7: Met	Mean	1.967	1.812	1.600	1.738	1.855	1.915	1.588	1.840
expectations	St Dev	0.758	0.620	0.545	0.537	0.687	0.519	0.492	0.543
Q8: Prior	Mean	2.559	2.574	2.211	2.268	2.420	2.570	2.438	2.429
opinion	St Dev	0.538	0.523	0.694	0.699	0.575	0.594	0.609	0.495
Q9: Opinion	Mean	2.075	2.029	1.897	1.977	2.014	2.084	1.824	2.111
now	St Dev	0.446	0.481	0.441	0.505	0.486	0.474	0.513	0.629

TABLE 6: MEAN RESPONSES AND STANDARD DEVIATIONS BY SELF-DEFINED ETHNICITY

Background	Sub-category	Self- defined Ethnicity
		Code
Asian	Indian	A1
	Pakistani	A2
	Bangladeshi	A3
	Any other Asian Background	
Black or Black British	Caribbean	B1
	African	B2
	Any other Black background	B9
Mixed	White & Black Caribbean	M1
	White & Black African	M2
	White & Asian	M3
	Any other mixed background	M9
Chinese or other	Chinese	01
ethnic group	Other ethnic group	02
White	British	W1
	Irish	W2
	Any other white background	W9
Other responses	The person did not	N3
	understand	
	The person declined to	N4
	define their ethnicity	

Appendix E: Self-Defined Ethnicity classifications

These codes are defined by the Office for National Statistics (ONS, 2018).

Appendix F: Sample Consort Flow Chart and Checklist



CONSORT 2010 Flow Diagram



Section/Topic	Item	Checklist item
Title and abstract	1	Identification as a randomised trial in the title
	16	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)
ntroduction		
Background and	22	Scientific background and explanation of rationale
objectives	2b	Specific objectives or hypotheses
Methods		
Trial design	33	Description of trial design (such as parallel, factorial) including allocation ratio
	36	Important changes to methods after trial commencement (such as eligibility criteria), with reasons
Participants	4a	Eligibility criteria for participants
	46	Settings and locations where the data were collected
Interventions	5	The interventions for each group with sufficient details to allow replication, including how and when they were
		actually administered
Outcomes	6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they
		were assessed
	66	Any changes to trial outcomes after the trial commenced, with reasons
Sample size	Ta	How sample size was determined
	7ь	When applicable, explanation of any interim analyses and stopping guidelines
Randomisation:		
Sequence	8a	Method used to generate the random allocation sequence
generation	86	Type of randomisation; details of any restriction (such as blocking and block size)
Allocation	9	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers),
concealment		describing any steps taken to conceal the sequence until interventions were assigned
mechanism		
Implementation	10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions

	116	assessing outcomes) and how If relevant, description of the similarity of interventions
Statistical methods	12a 12b	Statistical methods used to compare groups for primary and secondary outcomes Methods for additional analyses such as subgroup analyses and adjusted analyses
Results Particinant flow (a	133	For each moun, the numbers of narticinants who were randomly assigned, received intended treatment, and
liagram is strongly		were analysed for the primary outcome
ecommended)	13b	For each group, losses and exclusions after randomisation, together with reasons
Recruitment	14a	Dates defining the periods of recruitment and follow-up
	14b	Why the trial ended or was stopped
Baseline data	15	A table showing baseline demographic and clinical characteristics for each group
Numbers analysed	16	For each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups
Dutcomes and	17a	For each primary and secondary outcome, results for each group, and the estimated effect size and its
estimation	17b	precision (such as 95% confidence interval) For binary outcomes, presentation of both absolute and relative effect sizes is recommended
Ancillary analyses	18	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory
farms	19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)
Discussion Imitations	20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses
Seneralisability nterpretation	21 22	Generalisability (external validity, applicability) of the trial findings Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence
Other information		
registration	23	Registration number and name of trial registry
rotocol	24	Where the full trial protocol can be accessed, if available
unding	25	Sources of funding and other support (such as supply of drugs), role of funders

Part of